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MEDICAL PRIZE ESSAY.

A premium was offered for the best essay on the Suppression of Hemorrhage, to be published in the Medical Recorder; in conformity with that arrangement, we have carefully read the following essay, bearing the motto, "Non res fallaces sed res probatæ;" and are of opinion that it is entitled to the proposed premium. On opening the sealed note accompanying it, the author was found to be Horatio G. Jameson, M. D.

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ART. I. *Observations upon Traumatic Hemorrhage, illustrated by Experiments upon living animals.* By HORATIO G. JAMESON, M.D., Surgeon to the Baltimore Hospital.

"Is not this fear of hæmorrhage always uppermost in the minds of young surgeons; were this one danger removed, he would go forward in his profession, almost without fear." (John Bell.)

Non res fallaces sed res probatæ.

THERE seems to have been a degree of carelessness, among professional men, as respects the application of the term hemorrhage. We are in the habit of speaking of this phenomenon in a very general sense, and without the employment of precise terms, by which to designate whether the hemorrhage be the effect of some internal disease, or external wound. Dr. Cullen treats of active and passive hemorrhagies, for which there seems to be some foundation, in that form of hemorrhage which comes most properly under the notice of the physician; but to the surgeon, such distinction, though not altogether useless, is of less moment. In the present attempt at affording some improvement in this department of surgery, limited, as we are, to the confines of an essay, we shall aim at rendering this paper practical in its tenour;

and shall, by way of contradistinction to the more usual loose way of applying the term hemorrhage, employ the term traumatic hemorrhage; and confine our observations, as nearly as the subject will admit, to cases which come under that designation. The following arrangement, it is expected, will suit the subject of traumatic hemorrhage.

1. Physiology of the sanguiferous system, so far as relates to traumatic hemorrhage.
   a. Of the vessels.
   b. Of the blood.
2. Pathology of the sanguiferous system, so far as concerned in Traumatic hemorrhage.
   a. Of the vessels.
   b. Of the blood.
3. Of the means by which traumatic hemorrhage is to be arrested.
   a. By ligature.
   b. By compression.
   c. By retraction and contraction.
   d. By cautery.
   e. By astringents.
   f. By caustic.
4. Of the collateral circumstances, which favour the healing of wounded arteries.
   a. Of medical treatment.
   b. Of regimen.

Physiology of the sanguiferous system.—Of the self-evident fact, of the perpetual action of the heart, by which, principally, the blood is caused to be moved throughout the animal system, it would be useless to say one word, in treating upon our present subject, were it not that some of the greatest men have ascribed the moving power entirely to the heart; and the still more important consideration, that in the treatment of wounded arteries, we must, to a certain extent, control that important organ in its action; of which we shall speak in our fourth division. The opinion of the illustrious Harvey, since advocated by Bichat, in favour of the heart possessing powers sufficient for the circulation of the blood throughout the arterial system, did much towards retarding the more correct opinion, which has since been fully established; and for which we are especially indebted to John Hunter, and also to Richerand, John Bell, Magendie, and many others. To practical men, it is not at all important

to decide whether Mr. Hunter's opinion, that the muscular structure of the arteries is principally instrumental in the circulation, or whether, according to Magendie, and others, it is the result of their elasticity. Mr. Hunter says, vol. I. on Inflammation, p. 99, that the elasticity of arteries does not depend on life. This opinion requires some correction. The phenomena which have been noticed by Mr. Hunter, and many since his time, that arteries will not bleed when materially injured, and the wonderful degree of elasticity which is found in any part of an artery torn out of a living animal, which will be found in our experiments, prove to our satisfaction, that even the elastic power of arteries is greatly lessened after the death of the animal; and, also, wherever an artery is much injured. The opinion advanced by Mr. Hunter, that in the larger arteries, elasticity is chiefly concerned in propelling the blood, and the muscular power in the smaller arteries, derives support from the fact suggested by Richerand, in his Physiology, (p. 194.) "If a finger is introduced into the artery of a living animal, its parieties compress it in every direction." Now this contraction can only be attributed to a living action, since bodies having mere elastic powers, will only exert that power to overcome some extending force: and it is to the living artery only that Richerand ascribes this contraction upon the finger.

For practical purposes, it is enough to know, that the blood passes through the arteries with extreme rapidity; and that wherever an artery is wounded a loss of blood will take place, with more or less rapidity, and endanger, or destroy the life of the animal. Much pains have been taken, by physiologists, to ascertain the nature of the phenomena attending the operations of the animal economy, in closing up wounded blood vessels; and vast improvements in modern surgery have been the result. As we intend this paper to be practical, we shall avoid speculation, but to the surgeon it is highly important, that he be informed of the most important of the various interesting facts, which have resulted from physiological researches, in this branch of medical science, from which will necessarily be derived some sound doctrine and practical rules. Let us then proceed to examine some of the more important phenomena, attending traumatic hemorrhage, and the structures associated with it.

Of the power of the arteries.—Mr. John Hunter has satisfactorily proven that arteries possess the power, and do constantly, in their healthy state, contract in aiding the circulation of the blood, both in their transverse, and longitudinal

direction; and M. Richerand in his physiology (196) mentions the experiments of M. Lamure, to show that, much of the apparent motion in the pulsating artery is owing to its change of position, in the longitudinal direction of the vessel. Magendie in his physiology, (358) says that there is much motion in the longitudinal direction of an artery, which has been called, by some, the locomotion of the artery; and that it is especially observable at the aorta; hence the facility with which a divided artery will retract, and in this J. Hunter agrees, that retraction is the effect of elasticity, but ascribes contraction principally, though not wholly, to the muscular power in the coats. Of this power of retraction and contraction, which is sometimes sufficient to arrest hemorrhage, from arteries of considerable size, more especially when aided by compression, we shall have occasion to speak under another division; but let not the young surgeon overlook the fact, that, in some cases, owing to an artery being divided near one of its more particular attachments to fibres of fascia, it will retract but little, and sometimes not at all: whereas, in other cases they will retract, both in wounds and amputations, so as to render it difficult to take up the vessel. Charles Bell in his Operative Surgery, (387) calls our attention to the circumstance which will, now and then, be met with in practice, of small vessels bleeding freely within a bone; and which will bleed the more freely in proportion to its size, compared with other arteries, because it is less injured; as well as because it cannot retract: such an artery will often cease its bleeding upon our thrusting in a sharp pointed probe, to wound it and lessen its attachments, and thus enable it to retract: J. Hunter (on Inflm. vol. first 98) speaking of the elastic power of arteries, says, "this is exactly the reverse of the muscular power," but elsewhere says that elasticity does not depend upon life. The following observations of Mr. Hunter (107) seem to be important; "an artery has a check to its yielding, to so great a degree, and is stopt at once when stretched to a certain point, which is occasioned probably, by the muscular together with the internal elastic coat." This goes to prove, that the elasticity of arteries is in their *tissue arteriel,* of which we shall now speak.

The term *tissue arteriel,* employed by Desault and others, has been happily chosen, and we shall have occasion, when we come to speak of the use of ligatures, again to use this term. Under the present division, we wish simply to remark, that it serves to express that part of an artery which

so strikingly makes it differ from a vein, lymphatic, or duct —to which it owes all its active powers, according to M. Magendie, and many others; and that of elasticity, according to J. Hunter, Richerand, &c. Whether we admit with Richerand that the animal system may be resolved into four elementary structures, or the more extensive arrangement of Bichat, we may assert that the arterial coats are made up of cellular tissue, and differ from veins, in having in their tissue arteriel, a peculiar matter, which is to the artery what the parenchyma is to a gland: remove this, and a little muscular matter, and you leave a tube formed of cellular tissue. When an artery becomes obliterated, the vessel is resolved into its constituent cellular structure, as the tissue arteriel becomes absorbed. But since the term tissue arteriel seems to convey the idea of its being one of the coats of an artery, we have, with a view of obtaining greater precision of language, adopted the term materia arteriarum, by which we mean, so much of the middle or elastic coat of an artery as is absorbed, when the vessel is obliterated.

M. Richerand, (page 207) has observed that as the lining membrane of the arteries and veins have their origins alike from the heart, that we have good reason for believing, that they are in reality similar in their organization. We believe there is nothing contrary to this opinion, in all we know of the inner coat of the vessels, when carefully examined. It is true, Mr. Travers has pointed out a difference in the manner of obliteration between an artery and a vein—the former healing by the first intention; the latter by a thickening of its coats, by the deposition of lymph between them.* This I believe to be true, and my belief is founded on experiment and observation, but it arises, in part, from a difference in the structure of the vessels, independent of their inner coat; and, in part, from a difference growing out of circumstances necessarily attending the obliteration. Thus when we tie an artery, its contractile power is exerted in bringing the sides inwards, which lessens the caliber, and, pressing the coats firmly against each other, there is considerable resistance to the deposition of lymph between them, and the inner surface, a small distance from the ligature, as well as under it, is brought into contact by the ligature, and, thereby, the fullest effect is given to the first effusion of lymph to produce a union of the walls; besides we may reasonably suppose that the artery supplied with the blood in its fullest formative powers,

* To this there are perhaps a few exceptions.

will heal better than if filled with blood, deteriorated, as it is in the veins. On the other hand, a vein having no contractile powers, lies flaccid, when tied, on that side of the ligature nearest to the heart; and becomes distended and enlarged, by the pressure of the blood, on the other side of the ligature. Then the distention on the one side, by a coagulum of blood, prevents the approximation of the parietes; and the flaccidity of the coats on the other, invites a free deposition of lymph between the coats, and thus does nature in her works, provide the means of obliteration, notwithstanding the uncongenial properties of the blood within the vein. All this accords with our experiments upon the vessels in living animals, and we have found that if we merely interrupt the flow of blood through an artery, and irritate the coats, so as to produce inflammation, we shall cause the artery to close more slowly, and somewhat similar to what we see in the veins, by thickening of the coats, and obliteration, without actual adhesion of the inner coat. All which will be more fully illustrated in the detail of our experiments.

J. Hunter (117) says, that "all vessels in animals endeavour, as much as possible, to adapt themselves to the quantity of fluid circulating in them." This is more especially true as respects the arteries, and were it not for such a provision, animals would often die from the loss of a small quantity of blood. But we must not lose sight of the fact, that though the vessels have this power, and by it animals are often saved from death, that it is nevertheless but temporary, and mostly succeeded by debility and dilation of the vessels. If a patient be subjected to a great loss of blood at once, or by repeated smaller quantities, we shall have sooner or later an increased volume in the radial artery, which would often deceive the most skilful, were he not apprized of the loss of blood. We have heard of such mistakes, and well remember a case where a boy having wounded an artery, in the plantar arch, bled violently from day to day, for about two weeks, notwithstanding a practitioner had used all the usual means of compression. When called to the case, we took up the posterior tibial artery, and compression now succeeded in completely arresting the hemorrhage, but although the patient had lost so much blood as to disable him, for four days after the operation, to set up at all, and although his face was as pale as death, his pulse was full and frequent; and though very compressible when closely attended to, yet, without the greatest attention, one would have supposed there was high excitement.

M. Magendie, (357,) tells us, that if we apply two ligatures to a living artery, and open the vessel between the ligatures, the blood will spirt out: and if the vessel has been measured, it will be found considerably smaller after it is emptied. We merely cite this as an interesting experiment, calculated in a strong light, to support the opinion of the contractile power of arteries, a thing, however, we would suppose hardly requiring proof, since it seems to be self-evident. We have daily corroboration of this fact in the operation of bleeding, when we open a vein, where there is not much excitement of the vessels, it is only by considerable exertion of the muscles, in squeezing something in the hand, that we can cause the blood to flow freely. This fact seems sufficient of itself, to establish the belief of arteries propelling the blood independent, in some degree, of the heart; but, when we add to this, the many other circumstances which have been pointed out by different respectable writers, where shall we find room for doubt? The increase of the uterine arteries in gestation—of the increased circulation of the male and female genital organs in their state of excitement—of the branches of the carotid in the stag, in the season of the growth of his horns, as they are renewed from time to time—and, also, in the growth of tumours, and in irritations and inflammations of different parts of the body, lead irresistibly to the conclusion that the arteries have propelling powers.

The arteries become obliterated in their caliber, whenever the blood ceases to circulate through them.—Dr. Jones has been supposed to have much improved our knowledge of the physiology of the arteries, by his experiments, and thus laid the foundation of a more correct practice. But he has unfortunately blended with his actual improvements some serious errors. And in addition to his errors, associated with his own premises, certain changes in the employment of means, for arresting traumatic hemorrhage which he proposes, render some of hi views inapplicable, and some of his measures improper: besides all which, as regards the physiology of the arteries, he has added nothing material, to what was previously offered to the profession by Mr Hunter. That arteries will heal when the blood ceases to circulate through them, is a thing universally known; and it is equally well known, that this may be effected by compression, in various forms, by plugging, tying a mass of fleshy structure around them, by diversion of the blood through other vessels, as in the umbilical arteries, and their immediate associates; and, also, the ductus arteriosus, &c. So innumerable

are the instances of this kind, that the opinion of Dr. Jones respecting the necessity of cutting the inner coats of arteries with our ligatures, is truly surprising, the more so, as it has obtained the very general approbation of the profession. Among those who have ably aided in refuting this visionary notion may be placed first, A. Burns, and C. Bell; Scarpa, Crampton, and Travers. This subject will however come most properly under my third division when speaking of the use of ligatures.

Arteries will, within reasonable bounds, exert more force in proportion to the resistance they have opposed to their action.

Perhaps no position in physiology could be advanced, to which there would be so universal a consent, as that, in proportion as the muscular powers become invigorated, so will the arteries, in a proportionate degree, perform their office with more vigour; and, yet, we are well aware, that under such circumstances the muscles become more firm, and, viewed in their mechanical aspect, must more and more press upon the arterial tubes; and if these were merely passive conduits the blood would thereby be obstructed, whereas, owing to the living powers of the arteries, the muscles in this state of things facilitate the action of the arteries, which is a strong proof of their propelling powers. This state of things is calculated gradually to increase the powers of the arteries, and in the same gradual manner increase the impetus of the blood, while any particular cause of irritation, or the sudden stoppage of an arterial trunk, will more speedily augment the force of action in the surrounding arteries. How this is effected we cannot perhaps, at present explain, but daily observation convinces us of the fact. We may, however, venture to assert, that viewing the arteries as mere hydraulic tubes, upon which after tying a main trunk there will be a greater force of the circulating blood, under the influence of the heart, still this is not sufficient to account for the phenomena attendant. The vessels are at first prostrated, as in most other cases, where violence is done them, presently rising from a state of collapse, or apyrexia, reaction comes on, and we have, in the small vessels, a greater action than is natural to them. These phenomena are reconcilable, with, and indeed, they can only be explained upon the principle of the arteries having projectile, and other powers, in some degree independent of the heart, that is, they do not under all circumstances beat syncronously with the beats of the heart. J. Bell, Abridgment of Surgery, (77,) tells us, that,

"when we operate with a ligature upon an artery, we can mark the very instant in which it is obstructed; we perceive after a momentary coldness and deadness of the limb, a renewed action of its vessels, and increased heat." To this, I believe, there are but few exceptions.

This seems to be a suitable place to notice an interesting observation of Richerand. In his physiology, (198,) we have pointed out to us the fact, that where arteries pass through muscles the vessels are surrounded by a tendinous ring, as may be seen where the femoral artery passes through the tendon of the triceps, the aorta between the crura of the diaphragm, &c. Here we see, then, that while nature has made ample provision for the safety of arteries, where they would be liable to particular pressure, in time of great muscular exertion, no less wisdom has been employed, in endowing the arteries with the power of taking on increase of action, corresponding to the wants of the labouring system. But here we see that if nature has thus wisely provided for the maintenance of our lives and health, by giving powers of peculiar adaptation, we have also in it a proof, that we are not formed so as to ensure to us absolute safety from disease or death; since the very circumstances which we have pointed out as being suited, under particular circumstances, to be conducive to our welfare, may, under other circumstances, become the source of disease—how easily does the arterial system run into excess, both general and partial! yet, in thousands of instances, our safety depends upon this property.

Mr. Hunter, on Inflammation, &c. vol. I. (107) has informed us of the fact but little noticed, that "if an artery is cut, or laid bare, it will be found that it contracts by degrees till the whole cavity is closed;" and he has also told us that as you uncover an artery its pulsations become weaker. This agrees but ill with the notion that we must cut the arterial coats to heal them by ligature. This fact, so highly important in a surgical point of view, will require further attention hereafter. In its physiological aspect it is also interesting. Mr. Hunter has clearly pointed out the immediate economy of the arterial system, as regards their vascular structure under the head of vasa arteriarum. This has since received the particular attention of Dr. Jones under the name of vasa vasorum. It is not our intention to speak here of this part of our subject, but we have thought proper to pass it in review, by way of comparison, to what we wish to say concerning the circumstance, just cited from Hunter. Universal consent has been given to the opinion, that the welfare of

arteries depends much upon the integrity of their vasa vasorum; but have we not in some measure overlooked the parallel case, that every function, vascular, nervous, muscular, &c. are momentarily dependent upon the perpetual renewal of the blood, even to its application to every fibre and every atom? Admit this, and it will follow that as each pulsation gives a quickening impulse to the whole limb, every breach of continuity must lessen the vigour of the artery, so intimately associated with the surrounding structures; but, more especially, as the arteries act best when resisted by some reasonable force, and as they derive a mechanical support from the integument, fascia, &c. favourable to their action, then, by lessening this mechanical force, by the necessary division of parts, we take away a portion of that resistance so essential to the complete and vigorous action of the arteries—Whatever explanation we may adopt, there is no doubt of the fact, and hence, probably, has originated the circumstance, that few surgical writers say much about the pulsation of arteries as a guide in our operations. In many instances this has proved to us a source of perplexity, both in operations upon the human body, and upon living animals. We have, now and then, met with exceptions, but the young surgeon must not place much reliance on the pulsation of arteries, in difficult operations for tying them. In taking up the spermatic artery we have found this sometimes very perplexing, owing to the small size of those arteries.

We have already alluded to the circumstance of the resistance to arteries, leading to increased vigour in their action; we wish further to mention, the fact generally known, and somewhere particularly noticed by Mr. Hunter, we mean the increased force with which arteries will pulsate under tumours; extending a considerable degree of pulsatory movement, to tumours of tolerable consistance—the increased action of the arteries of the fingers, of one hand, if we press forcibly for some time upon them, with the other hand; and the motion which is given to the foot, by resting the ham of one leg, upon the knee of the other, so as to compress the popliteal artery—it is said a considerable weight may be appended to the foot, before the motion can be overcome, and yet if you expose the artery to the air, in a little time, but little pulsation, will in all probability, be felt—this we know, from experience, to be the case, in the carotids, both in the human body, and quadrupeds, to an astonishing degree. In conclusion, upon this item, Hunter has said, (166,) speaking of the pulsation of an artery, as felt through the

integuments; "were we to judge of the real increase of this we should deceive ourselves, for, when covered by the integuments, the apparent effect is much greater than it really is, in the artery itself, for in laying such an artery bare, the nearer we come to it, the less visible is its pulsation; and when laid bare its motion is hardly to be felt or seen." Again. "The more an artery is covered by a solid body the more is the pulsation to be seen, or felt."

Arteries will, in general, by their anastomoses support a limb after the main trunk is cut off.—This position has been so well established by experience, that we readily agree with the propriety of the remark which has been made by John Bell, that no surgeon should hesitate to tie any accessible artery in the human body, under circumstances requiring such an operation. Since Mr. Bell's publications, two operations have served to prove, that the arteria innominata may be tied without much apprehension of a failure, on the part of anastomosing vessels, to maintain the life of the arm, &c. Mr. Cooper's experiments upon dogs, and his operation upon the human body, afford a rational hope, that even the aorta may be obliterated with some hope of occasional success in the latter. Still, it must be recollected that several cases are reported, in which nature was not adequate to the task of establishing a new circulation, and mortification has been the result. It has been a prevailing opinion, that in cases of aneurism we should have less to fear, from want of power in the arteries, by waiting till there should be some obstruction in the trunk, and a gradual enlargement of the branches, as a consequence of such obstruction. This opinion has its advantages, in particular cases, but the experiments accompanying this paper, it is hoped, will suggest a method calculated to obviate such difficulty. We have already intimated, that by a law of the animal economy, arteries when they encounter moderate resistance, acquire increased powers; and called to mind the fact, pointed out by Mr. J. Bell, of the momentary prostration of the arterial action in a limb, upon tying an arterial trunk. Now, although the speedy reaction, which is correctly said, by Mr. Bell, to succeed a death like pause in such a limb is encouraging; and, although, we are strongly supported by our experience in the belief, that there is but little danger to be apprehended, in tying the main artery of a limb; still we believe that a share of increased confidence will grow out of the experiments, yet to be stated; and which method, by a gradual obliteration of

the artery, will not subject the arterial branches to the degree of prostration, which succeeds the tying of a ligature upon a trunk, which has sometimes led to the death of the limb. In Richerand's Physiology, (195,) it is said, that owing to the greater share of exercise to which the right arm of most persons is subjected, the subclavian artery is found to be larger, than that of the left arm. This fact is not only a confirmation of what we have already said, respecting the power of adaptation to circumstances in the arteries, but is important from its serving to suggest the belief, which scarcely can be doubted, that the branches will maintain a corresponding increase, with the decrease of the main trunk. This leads us to believe that we have less to fear as to the sustenance of the right arm, upon tying the innominata, or any of the great arteries, arising from it, than in the left.

Arteries requiring like other parts their own particular vessels, can only bear a separation, from the vessels Subsidiary to those vessels of nutrition, to a moderate extent.—We have already hinted at the circumstance of Dr. Jones having excited particular attention, by his interesting experiments; but to Mr. Hunter are we principally indebted for correct views of the vasa vasorum. If we do not mistake, however, both Jones, and those who have adopted his views have here fallen into error, in attributing too much importance to their fears of risk, as to the safety of the vessels, by insulating them from their subsidiary vessels. Indeed, we think, they have not been sufficiently explicit, in distinguishing the true vasa vasorum, from the vessels subsidiary to them. Great apprehensions exist, that by detaching an artery from its connections, to but a moderate extent, we incur the risk of sloughing of the vessel. This, to a certain extent is true, but we have in some degree lost sight of the fact, that the principal risk, in our operations upon arteries, is owing to strangulating the true vasa vasorum, while we at the same time, cut off the vessels subsidiary to these. The experiments of Mr. A. Cooper, and others, upon the carotids of dogs, in which those arteries were exposed and the fact noticed by Boyer, and known to many, that an artery may be insulated for some inches from surrounding parts in large suppurations, &c. and instead of sloughing, will granulate, and form adhesion to adjacent parts, is truly interesting, and leads to this conclusion; that there is but little danger in detaching an artery, even for two or three inches, if we do not strangulate, or cut off, the true vasa vasorum by

our ligature; and, hence, all the danger imputed to this circumstance, or nearly so, arises from the use of improper ligatures, of which we shall speak hereafter.* At present, we wish merely to say, that by the use of the leather ligature, applied moderately tight, we shall obstruct the flow of blood, and heal the vessel without producing sloughing of the coats, or even materially strangulating the vasa vasorum. But our present experiments will, it is hoped, afford a method divested of all possible risk, of what we have been alluding to: (viz.) That we can obliterate the arteries gradually, without surrounding them by ligature, and thereby not only do away the risk of injuring the artery, by detaching it or obstructing the vasa vasorum by ligature; but, we may, by the gradual closure of the artery, aid the anastomosing arteries by which the limb is to be supported, to enlarge as the trunk contracts. It may be necessary further to remark, that by the true vasa vasorum, we mean those vessels which will support an artery, throughout several inches of its extent, although it be separated from the surrounding parts; and sometimes even surrounded by pus—these cannot sustain an artery under the application of a common ligature, yet an artery having its vasa vasorum, and these supported by the vessels immediately subsidiary to them, will bear a ligature which cuts the coats, in part strangulates the vessels, and eventually, cuts through the whole substance of the vessel, by suppuration or attrition, and thus makes its escape from the wound, but hence a cause occasionally, of secondary hemorrhage.

It is not altogether the size of an artery, but its order, which renders its bleeding dangerous. This is not only true, as respects particular arteries, giving out a quantity of blood in a child which would be dangerous to it, but not so to an adult; but arteries of a particular order will bleed more freely in the same individual adult.

Charles Bell, Operative Surgery, vol 1, (412,) says, it is not so much the size as the order of an artery which will render a wound of it dangerous. When we add to this fact, what has been said by Richerand (194), that the arteries of the mammæ, of equal size, will bleed more than those of the toes, we shall have no difficulty in perceiving, that the arteries of the female mammæ resemble in some degree the arteries of the genitals, both being subject to peculiar and plea-

* Several interesting cases of veins being exposed by suppuration and not giving way, may be seen in Cooper and Traver's Essays.

surable excitement, of a sexual nature. In a word, the arteries of the mammæ in the one sex, and of the penis in the other, are endowed with a peculiar power, which may be termed their erectile action. And though we admit, that the healthy and natural excitement of those arteries, is only produced by some influence of the will or passions, still such is their economy that every morbid condition of those organs, in which irritation, or inflammation is set up, appears to be attended by increased action of the arteries, at all times, so easily excited. And hence the propriety of Richerand's remark, that arteries of the mammæ bleed freely. The opinion here advanced is fully supported by our own experience —we have frequently amputated toes and fingers, and, except in the great toe, have never had occasion to tie an artery; whereas in arteries of the penis, whether from wound or by mortification, we have always seen a degree of obstinacy, in some measure surprising—these vessels bleeding with singular freedom, and always requiring ligatures. We have seen several cases where the patients would certainly have bled to death, had the arteries not been tied—they were sometimes very small, mere vessels of the prepuce; and, in operations upon the breast, we have sometimes had hemorrhage to take place from arteries which gave out little blood until the dressings were applied, and the parts became warm. Whatever may be the cause, and whether our explanation prove satisfactory or not, we admonish the young surgeon to be on his guard, and never trust arteries of the mammæ, or the penis, which he can discover, during his operation, to remain untied. And Richerand requires correction, when he says (194,) we need not always tie arteries of the penis, in cases of their rupture in gangrene: we have seen some deplorable cases in which there was much danger, and let these be a solemn warning to others who may not have seen such cases.

Of the Physiology of the Blood.—We do not consider it necessary to enter into the inquiry whether, or not, the blood be possessed of vital properties. Indeed, our limits will not allow us space, more than merely to say that we approve, in their fullest sense, the sentiments of Mr. J. Hunter on this subject. Neither do we deem it necessary to attend to the question, to what extent, if at all, the blood is under the influence of chemical laws—we shall content ourselves by merely declaring as our opinion, that chemical laws are always at variance with the vital—be this as it may, such inquiry has nothing to do with our present subject.

It is chiefly necessary to be acquainted with that property of the blood, and of the lymph, which disposes them to coagulation, and to know that they will undergo that change, as well in the vessels, within cavities and deep wounds, as when exposed to atmospheric air. This wonderful phenomenon displays alike the wisdom and goodness of our maker, in providing for our safety, in cases of accidents, by which blood vessels are opened. Is an artery wounded, no sooner is the wound completely filled with blood, than it coagulates; and, if the vessel be not too large, a little pressure, by interrupting the blood, leads to its coagulation in the vessel—if the vessel is too large to expect its closure, by more firm pressure, or the application of a ligature, we check the flow of blood; and it is the property of coagulation, which prevents the blood from escaping from thousands of vessels. No sooner have we the hemorrhage arrested, by art, by the coagulation of the blood, or both, as the case may be, than the blood is resolved into its constituent parts, and lymph is made to surround every opening through which blood would escape. After the coagulated blood was dissolved, there would be a renewal of hemorrhage, if the lymph was not also coagulable, whereby it soon becomes viscid, dense, fibrous, vascular, and endowed with organic life; and, thus, by the formation of living parts, the opened vessels are effectually sealed. We have already seen, that the healing of wounded arteries is facilitated, by the contraction and retraction of them; and it will follow, that if there is a want of healthy action in the arteries, which would prevent their contraction, the coagulation of the blood would not be sufficient, to secure the eventual closure of the vessels. And, on the other hand, if the blood has become dissolved, as in malignant fever, or depraved, as in cases of scurvy, healing of the parts may be prevented, by the want of coagulation. There have been reported some singular cases of fatal bleedings, from very small vessels, as the finger, gums, sockets of the teeth, &c. and even extending to several members of the same family: but we do not know that the question has been clearly decided, whether this occurrence may be owing to some defect in the blood, or in the vessels.

We believe most surgeons at present consider it as a thing settled, that internal remedies cannot be, in any degree, relied on in cases of traumatic hemorrhage. We are inclined to believe that this matter has been too hastily decided, and is still entitled to the most careful attention. If we can, as I believe most practitioners will admit, in many alarming

cases of internal hemorrhage, by the use of astringents passed into the stomach arrest the discharge of blood, why should we despair of similar results, to a limited extent, in traumatic hemorrhage, under particular circumstances. We are aware that Cullen, Good, &c. in distinguishing hemorrhage into active and passive, (the latter only changing the terms to that of entonic, and atonic,) ascribe the difference to different states of the vessels; the first being that state in which the small vessels are ruptured, by over-distention, the latter owing to the blood escaping by the exhalant vessels, because of their relaxation. But do we not frequently see the blood containing too much serum, which renders it less disposed to coagulate: and the blood depraved, as well as the solids, in scurvy? Do we not occasionally see, in our operations, an indisposition in the blood to coagulate, especially in venous hemorrhagies, and that we can promote the coagulation by astringent applications? We have not room to discuss this question, neither do we deem it important, in a practical paper, to decide whether in cases of acknowledged utility from astringents, internally, or externally applied, our object is gained by the action of such articles upon the blood or solids. For our own part, we believe that in most cases we improve the condition of the solids; but since we see the happy effects of lead in arresting internal hemorrhage, and have known since the experiments of Mr. J. Hunter, that lead will coagulate the serum of the blood, there appears to be sufficient reason for believing that it is possible, that in some cases remedies may improve the qualities of the blood; and aid in arresting hemorrhage in depraved habits. In habits of great debility, in which the serum exists in undue proportion, we would expect benefit from lead in particular, also other tonics; and in scorbutic habits vegetable food and acids will greatly promote our wishes, and, that, by giving to the blood greater powers of coagulation. It may be said this will be effected by internal remedies acting upon the solids, we are careless about the theory, if the practice be admitted; but we hold, that there are occasional cases in which, even in traumatic hemorrhage, we should use internal means. These are principally, protracted cases in which there is a general oozing from wounded surfaces.

Mr. Hunter, vol. 1, page 15, says, "though fluidity is necessary to enable the blood to circulate, yet coagulation is no less necessary when it is to be disposed of out of the circulation, even within the body, and, therefore, deserves to be considered with no less attention." We wish merely to

say here, that we consider this a matter worthy our particular attention, but must refer to Mr. Hunter, and others, for the necessary investigation of the nature and uses of coagulation.

We are aware that in proportion as the individual ascends in the scale of more perfect health, so will the power of coagulation of the blood improve—that in proportion as a limb has its nervous energies injured by accident, or the whole nervous system by a stroke of lightning, &c. so will the tendency to coagulation be diminished in proportion as the vital powers retire, and gangrene approaches—that arteries of tolerable size have been seen to stand open mouthed, but did not bleed, because they were injured by some violence; still, all this is no more than a negative proof, that the defect may not in part exist in the blood, since we must admit that there is a close and intimate union between the vital state of the vessels, and of the fluid upon which they act; and which may be said to be the very matrix of the vessels themselves. The lædentia of the one, will interrupt the healthy action or state of the other, reciprocally; and the juvantia of either will be adjuvant to its associate.

In addition to more hurtful agents, cessation from motion will dispose to coagulation in the blood, and hence the advantage in practice of applying compression to small vessels; and of exposing bleeding surfaces to the influence of atmospheric air, and to cold applications; and hence too, the advantage in larger arteries of fainting. The experiments of Mr. Hunter lead us to believe, that blood will coagulate in vacuo, and that this tendency is not increased by heat. This does not militate, however, against the opinion, that cold, and exposure to atmospheric air, may be useful in promoting the coagulation of blood. All these agents act simply by lessening action, and lessening of action tends to coagulation—asphyxia acts upon the same principle. We must, here, carefully bear in mind that these agents exert but a temporary action, and if vessels of considerable size are opened, we must either avoid them, or wait till their influence has ceased, that we may find the vessels and secure them, by ligature, or compression, as the case may require.

Pathology of the Blood Vessels.—The pathology of the arteries, as connected with the subject of traumatic hemorrhage, seems to admit conveniently, of division into cases in which obstacles present themselves at the onset, against the stoppage of hemorrhage; and into those in which some hurtful agent or morbid condition of the parts concerned, dispose

to secondary hemorrhage. In the first division, we may notice ossification of the arteries, the wound or division of a large vein, and debility of the solids. In the second division, may be noticed as hurtful agents too free a separation of arteries from their connexions—improper ligatures, wrong application of ligatures, and wounds of the main vein of a limb. And as morbid conditions, may be noticed general fever, local inflammation, loss of nervous power, and disease of the artery to be operated on.

Ossification of vessels may present an obstacle to the stoppage of traumatic hemorrhage. Mr. J. Bell tells us, abridgment of his surgery (49), that the arteries "are sometimes ossified, they break or crack, when we attempt to bend them; and the drawing a ligature round such an artery tears it, from want of pliancy: our anatomical injections are only successful in young subjects; while in older subjects the arteries burst because they have lost their strength, or tear under the necessary ligature because they have lost their pliancy:" and also, "the surgeon often forsees, by the first touch of his finger, those burstings of the artery, and secondary hemorrhage of which so many have died." Scarpa, and others, confirm these descriptions of Mr. Bell. The question now naturally arises, how far are we to be governed in cases of ossification of an artery, in deciding upon the amputation of a limb, upon which may depend the life of the patient. Instances are not wanting in which this has been considered as a prohibition against amputation. But instances are not wanting where such arteries have done well. J. Bell (50) cites two cases from J. L. Petit, and Acvell, in which, notwithstanding the bursting of arteries, because they were ossified, the patients were saved by compression, though the blood could not be checked by the tourniquet. S. Cooper First Lines, (156,) says, "when the bleeding vessel is ossified or situated in a bony canal, a small portion of lint introduced into its orifice, will stop the effusion of blood." As far as our own observation goes we would say, that if there be such a thing as actual ossification of an artery it is very rare: all the cases of reputed ossification were rather earthy concretions, along the inner surface of the vessels, of which we have seen several cases. In some cases, of this condition of the radical arteries, so considerable as to conceal the pulsation; upon splitting, and stretching out upon a board, the artery taken from the subject, with the inside of the vessel upwards, the appearance presented, much resembled rough sanded paper. Though these cases may sometimes be attend-

ed with induration, or disease of the inner coat; yet in most cases of this kind, we believe, the coats remain but slightly diseased, except in little spots here and there along the inner surface: and this seems to agree with the observations of Scarpa. So true is this that in many cases we can tie the vessel, by means of a soft leather ligature, without breaking its coats; in other cases, and of this we may judge by pressing the end of the artery between the finger and thumb, we may determine whether this procedure would be proper; or whether we had not better plug the artery, for which we would advise a roll of leather, to be used as a stopper, instead of lint. By stopping the flow of blood, if it be not too near a branch, the blood will coagulate, inflammation will succeed, the incrustation will be thrown off, granulation will arise within, (the admission of the opinion by Bichat, that the inner coat will not granulate, is no objection to what is here said;) and, the outer coats in this case, no longer prevented by the concrete contracts, and thus is the vessel obliterated. We should not in any case, other circumstances being favourable, refuse to amputate either of the limbs, on account of the ossification of the vesssels; plug them with care, and nature will obliterate them. We consider the coagulum, in all cases, as serving no other purpose than that of a stopper to the artery, till inflammation, &c. may close the vessel, and, therefore, should there be no coagulum, owing to the proximity of a branch, the wound will lead to inflammation, by which the vessel is to be closed: and while this is going on, the stopper will prevent hemorrhage. Absorption will take place: and the substance of the arteries with the exception of the cellular structure as in other, or common cases, will be removed.

Of a wound or division of a large vein as presenting an obstacle to the stoppage of hemorrhage. In the Surgical Essays of Cooper & Travers, we have a very interesting account of the physiology and pathology of the veins by the latter. The extensive experience of this gentleman led him to these conclusions—that, notwithstanding the numerous deaths from the application of ligatures to veins, a very considerable number of which he presents to view, still the veins are among those textures which take on inflammation slowly; but, when it is excited, is liable to run into excess, and leads to danger. That veins do not heal by the first intention, as was supposed by Mr. Hunter, Bichat, and many others, but by so abundant an effusion of coagulable lymph as to choke up the vessel; this leads to a coagulation of blood in the vein,

and to an effusion of lymph, between the coats and around the vessel, by which it is converted into a cord-like substance: Or if a ligature has been applied, a coagulum will form, on the side of the extremity; and the phenomena just pointed out will take place, next the heart; and close the vein. This accords with our experiments with one exception. As regards danger, from wounds of the veins taking on inflammation, there does not appear to be much difference as to the extent of the injury, the simple wound of blood-letting leading to the same mischief, as results from ligature, or severer injuries; and the same remark applies to adjacent inflammation, suppuration, &c. and, indeed, Mr. Hunter has seen inflammation of surrounding parts, extending by continuity to the coats of veins, and leading to inflammation, effusion of lymph, to suppuration, &c. within the vein. What we have said respecting the veins has no direct connexion with our subject, but as there is more danger to be apprehended from injuries to some of the veins than arteries, the young surgeon should be apprized of every possible source of danger. In addition to the particulars already pointed out, and not directly connected with our subject, is that arising from the obliteration of a vein of great size. The blood coming here from smaller into larger trunks must stagnate and become a source of danger: and we have already alluded to the fact of the blood within the veins having been, in a great degree, divested of its formative or directly sanative properties, before it arrives at the larger trunks. It is true the blood under such circumstances will coagulate, but so far as we have had opportunity for actual observation, the veins become inflamed on the extremity side of the obstruction; and, therefore, while we are willing to admit with Mr. A. Burns, that the wounding of the jugular vein would not be extremely dangerous, we are not less convinced of the truth of the opinion of Boyer and others, that to lose the principal vein high up in either of the extremities, is almost certain to occasion the loss of a limb, or the life of the patient. In the first case, there is not only two veins from the head, but there is a singular provision against accident to the brain, from obstruction of the jugular of either side; in the arrangement of the sinuses within the head, aided in slight degree, by the external jugulars. In the latter cases there is, at the upper extremity of the limb, little more than a single conduit for the returning blood. Many cases of obliteration of the jugular, on one side in the human subject, from the pressure of tumours, &c. are to be seen on record: one case of which we have seen. C.

Bell, (385,) speaking of rupture of varicose veins, says, death has been the consequence, in some cases, of a sudden loss of blood, from the femoral vein in a varicose state.

Debility of the solids may prevent the stoppage of hemorrhage.—Richerand, (190,) informs us that he saw a case, in which there was secondary hemorrhage, in the stump of an amputated thigh, on the nineteenth day, occasioned by a scorbutic habit. And the same author, (202,) has seen the capillaries so relaxed in scurvy, as to produce effusion of blood into the muscular structure. Many authorities might be added, were it deemed necessary. We will simply remark, that it agrees with the experience of Boyer and ourselves; but we do not confine our observations to scorbutic habits merely, but in habits strongly predisposed to atonic hemorrhage; this will become, in many cases, a source of trouble and danger; apt to show itself, not only in cases where injury of the nervous power has been added by violence done to a part, to the debility of habit we have alluded to, but also in amputations, in operations upon the rectum and bladder. We shall avail ourselves of the excellent advice of Dr. Good, in the fourth division of our subject, and wish merely to remark, that in vol. II., (469,) this author advises mild stimulants, in cases of atonic hemorrhage. The success attending this practice, serves to confirm the correctness of the theory of this author and Cullen, respecting passive, or atonic hemorrhage.

The too free separation of an artery from the connexion between the true vasa vasorum, and the vessels immediately subsidiary to them, may lead to secondary hemorrhage.—Mr. Abernethy, in his work, vol. I., (147,) alluding to the want of success which attended Mr. Hunter's operations for aneurism, says, "the hemorrhage which took place after the operation, in the first cases in which it was performed, arose from the ulceration of the artery that had been tied. The vessel, in these cases, was laid bare, and detached, in some degree, from its surrounding connexions, and the middle of the detached portion was tied by a single ligature. An artery, thus circumstanced, must inflame; which it would do, in different modes and degrees, according as the state of the constitution, or of the part, was more or less healthy; and this inflammation produced the ulceration of the vessel." There seems to be almost an universal consent among surgeons, at present, as to the danger of detaching the artery too freely; and, indeed, before this opinion was strengthened by the interesting experiments of Jones, Mr. J. Hunter was aware of

the error in his first operations. The weight of Abernethy's name, gave very general currency to the belief, that the operation for tying an artery was rendered more safe by applying two ligatures, and dividing the vessel between them. Our own actual experience, observation, and reading, serve to support the opinion of Mr. Abernethy, when the whole matter is considered in relation to the circumstances before him, at the time his opinion was made up. But we trust this matter has assumed a new aspect, since the introduction of the animal ligature, by our countryman, Dr. Physick. We shall, in place, examine into the advantages of the different methods of applying ligatures. We deem it sufficient, in this place, to say that, other circumstances being favourable, we have nothing to fear from secondary hemorrhage, provided we apply a single ligature of a proper kind, and in a proper manner. And that sufficient attention does not seem to have been paid to the fact, that arteries may be bared for a considerable distance, both by the knife and suppuration, without causing any material injury to the vessel; and that to occasion the death of an artery, we must not only detach it, but also strangulate its vessels. We hope to prove, that a ligature may be so applied to blood vessels, as to avoid an entire obstruction in the true vasa vasorum, and thereby the risk will be much lessened. Still we are far from wishing to be understood as speaking of this as a matter of little importance. We are well aware of the advantage of disturbing an artery as little as possible, in our efforts at applying a ligature round it.

Mr. Abernethy has properly said, that the inflammation consequential to the application of the ligature to an artery, under certain circumstances, will appear "in different modes and degrees, according as the state of the consitution, or of the part, was more or less healthy." There can be no doubt of the correctness of the opinion, that peculiar conditions of general habit, or local disease, may lessen the chance of healing an artery, under the application of a ligature; but even under the most unfavourable circumstances, we believe a proper ligature, well applied, will secure the healing of the main trunks; and that under the most favourable circumstances, as to the manipulation in the application of the ligature, it is only a general oozing we have to dread. The extensive wounds which we so frequently see doing well, notwithstanding the destruction of vessels of considerable size, and sometimes where there is but little vigour of consitution, as well as our experience in surgery, all serve to convince us

of the soundness of the structure of the human body, under ordinary circumstances. Mr. Travers tells us, that Mr. Coleman, a professor in the veterinarian school, in London, assured him that in the horse, even small wounds of the skin would seldom heal by the first intention. Our experiments upon animals, induce us to believe that the remark of Mr. Coleman will apply to all brute animals, upon which we make experiments upon the blood vessels. Few wounds heal well by the first intention, and, indeed, it would seem to be a fair inference to make, a priori, that the structure of animals destined to live from one-fourth to one-half of the period allotted to man, would be less perfect, and less capable of bearing violence done to it, than the more complete structure of man. The presumption then is, that in a vast majority of cases, indeed almost without exception, if we tie an artery, the coats of which are healthy, in a proper manner, it will heal. It will follow then, as our experience has already gone far to prove, that secondary hemorrhage is, in ninety-nine cases in the hundred, owing to bad surgery.

Improper ligatures will much increase the risk of secondary hemorrhage.—Mr. A. Burns, (147,) says, ligatures sometimes produce disease in the coats of an artery, and prevent union. And, (152,) this author ascribes secondary hemorrhage to a diseased gland being seated near a ligature upon a vessel; that, in some cases, sinuses form and insulate the vessel. We have no doubt this view of the subject was correct, under the former state of things. But we more than doubt that the same view will not apply to an artery carefully tied with an animal ligature. We are also told by Mr. Burns, (153) that tying a nerve in with a ligature, may produce secondary hemorrhage, owing to the ligature not coming away, the indestructible nature of the nerve preventing it. The irritation of a rough ligature, continued a long time about the vessel, together with the attempts which are generally made by surgeons to get away the ligature by pulling, leads to suppuration, and erosion of the coats of the vessel. We hesitate not to say, that no such consequences will follow the use of the animal ligature, though we would disapprove of tying a nerve where it could be avoided; but we can easily imagine cases, in which a surgeon, for want of assistance, might be compelled to dip down a suture needle, and enclose muscles, nerves, and every thing around, with the bleeding vessel; and we venture to say, that perfect safety will attend the use of the animal ligature, under such circumstances.

It has been held by Dr. Jones, and his followers, as a thing

almost beyond doubt, that we must cut the inner coats of an artery to secure its obliteration, of which we have already given our views. But it seems proper here to advert to the fact, which we have ascertained by experiments on the arteries of recent subjects, that in the larger order of arteries, to which we apply ligatures, we cannot cut the coats equally around, nor can we, by cutting a ligature, bring the sides of such arteries into contact; they, like the larger veins, form plaits, or rugæ, and the highest points of those rugæ will be cut through, while the lower points will not be touched by the ligature. And we have no doubt, but that in applying a ligature to the arteria innominata, secondary hemorrhage will, in most cases, succeed the use of a cutting ligature. But even here apply the animal ligature, and you can bring the sides of the vessel pretty smoothly into contact, without entirely obstructing its true vasa vasorum. The artery will, in the first place contract and close, by a thickening of its coats; and thus its obliteration will be accomplished, without producing suppuration of the arterial coats, or cutting off the vessel by the ligature; and eventually, we shall have some extent of the vessel reduced to its primary structure, (to wit,) an imperforate fold of cellular structure.

We do not deem it necessary to go into the detail of the views and experience of different writers, which have been quoted by Mr. S. Cooper, in his First Lines, respecting the preference due to the different ligatures in use. Let it suffice to say, some men high in the profession do not approve of the fine silk ligature, of Mr. Lawrance and others—because they sometimes produce irritation, and frequently, after cutting the vessel in two, they come out of the wound; and because to very large arteries they are inapplicable, owing to our not being able to draw them sufficiently tight, to close in the sides of the vessel. Upon the whole we think much credit is due Mr. Lawrance, and some others, for the introduction of the fine silk ligature into practice, since before the use of the leather ligature it was certainly a very great improvement; but, at the same time, we must declare as our opinion, that this ligature was only suited to arteries of moderate size; and that when you enlarge the ligature of silk, to suit arteries of the first order, it possesses no advantage over the common flax ligature, since both are alike indissoluble, and indestructible by the absorbents. Here then we see the superiority of the leather, which by the time it has answered the purpose of obliterating the vessel, is dissolved

by the fluids, and enters with them into the absorbing vessels; of which we have had actual knowledge.

Of the ligatures of reserve applied by the French surgeons and some others, (even Mr. Hunter, attempted to avail himself of this method,) we have little to say. A few remarks from Mr. Abernethy, may serve to express our opinion. "The occurrence of hemorrhage, led some surgeons to adopt a practice which cannot but be considered injurious. They applied a second ligature above the other, leaving it loose, to be drawn tight if the first should not answer. The second ligature must not only keep a certain portion of the artery detached from the surrounding parts, but must also, give additional irritation to the inflamed vessels, and on both these accounts, it is more likely to make the inflammation end in suppuration, or ulceration." What shall we think of those who applied three or four ligatures.

The method proposed by Scarpa, Crampton, &c., of using broad ligatures, temporary ligatures, interposing rolls of linen &c., all lose their importance, when compared with the animal ligature. These are all liable to considerable objection, on account of the necessity of their removal; when the object of closing the vessel has been accomplished, which can never be done without a risk of producing irritation, sufficient to overbalance any advantages this method may possess, over the cutting ligature.

Of the improper application of the ligature, as a cause of secondary hemorrhage.—Mr. A. Burns tells us, (155.) that he has seen one case, "where the surgeon tied the sheath formed in the substance of the triceps muscle, mistaking it for the femoral artery, which had retracted itself." As soon as the patient would be moved, in such a case, or the circulation recover its vigour, there would be a dangerous, or perhaps fatal hemorrhage. In the case mentioned the patient was nearly destroyed, and only saved by the immediate application of the tourniquet. Dr. Jones tells us, (see Boyer's Surgery, by Dr. Stevens,) that where a ligature fails to answer the object intended, it is either "from the clumsiness of the ligature, which prevents its tying compactly and securely round the artery, or from its not having been applied tight enough, lest it should cut through the coats of the artery too soon; or finally, from its having that very insecure hold of the artery, which the deviation from the circular direction must necessarily occasion." Mr. S. Cooper, (144,) says, "I have known the ligature to slip from not having been skilfully applied; I have known it pulled off by an accidental jerk of the hand,

and hemorrhage take place. I can even suppose an artery may be so forcibly tied, as actually to produce a direct laceration of the external coat." We believe that the animal ligature will secure the patient from all these dangers, except one, (to wit,) the awkwardness of the surgeon; and even in this respect, the animal ligature is preferable, not requiring that precision of management essential to the cutting ligature. If the ligature is cut from the leather with care, it will always admit of being tied sufficiently tight, but can never be made to cut the coats, provided it is made of soft buckskin, and not hardened by drawing it. It is less likely to slip when somewhat insecurely applied, because being elastic and soft, it is spread over a small space of the vessel, almost immediately adheres by its glutinous qualities; it lies more securely, while the cutting ligature resting on a mere line, and having neither adhesive properties, nor the advantage of a small vacuum between the vessel and the ligature, as is the case with the flattish adhesive ligature, is more likely to slip off. Besides, as we cut off the ends close, there is no risk of pulling them away by an "accidental jerk of the hand." In support of these assertions, we have to offer the experience of several years practice, during which we have used no other than the buckskin ligature; and no such thing as a secondary hemorrhage has ever occurred in our practice, extending alike to cases of aneurism, tumours, and amputations. We are, moreover, decidedly of the opinion, that in no case whatever have we had reason to suppose that the healing of a wound, accidental, or surgical, was delayed by our ligatures: we never see any thing of them after their application. C. Bell, Oper. Sur. vol. 2., (414,) "I have twice seen the radial nerve included in the ligature put around the brachial artery." He advises us to flex the limb to avoid this accident. The animal ligature would, we believe, render this a matter of little moment.

A large wound or division of the principal vein of a limb, may lead to difficulty, danger, or secondary hemorrhage— On the subject of wounds of the veins, there appears to be a paucity of information. The experiments of Mr. Travers, on ligature of the veins, &c., are highly interesting, but do not throw much light upon the subject of wounded veins, so far as relates to hemorrhage. The generality of surgeons agree with Boyer, when he says, (124:) "If the vein be cut across, the circulation through it ceases, and the blood passes through the collateral branches." What he says further is

equally important, and we believe less attended to; the hemorrhage from a large vein may become very dangerous, when the vein is the only one belonging to the limb; for then the compression, or ligature necessary to staunch the blood, hinders its return to the heart, and the member swells so prodigiously, that gangrene may supervene. For example, if the femoral vein were opened at the groin, above its junction with the saphena vein, there would result from the compression necessary to stop the bleeding, a great tumefaction, and its necessary consequences. It is obvious that nearly the same thing will occur from division of the great brachial vein, where it enters the axilla. So important do we consider this matter, that we believe cases do sometimes occur in which the loss of the vein will require amputation, or the patient will perish. It is generally known, and ably proven by Dr. Jones and Mr. Travers, that small wounds of veins, excepting a case now and then of inflammation of the vessel, are not dangerous; and the obliteration of a superficial vein of a limb, with the same exception, will do no material injury—the collateral vessels in such cases, serving to carry the blood to the heart. And Mr. Hunter had no hesitation in advising compression upon an inflamed vein, with a view of procuring adhesion of its sides. In support of the position that the division of a principal vein, at the upper end of a limb, may prove dangerous we shall briefly detail two cases which came to our notice.

A healthy young man had been wounded in the thigh by a musket ball—at the end of three weeks, the femoral artery sloughed, and the patient was only saved from imminent peril by the presence of a surgeon, who instantly made pressure. Dr. ——, was called on, and immediately proceeded to tie the artery above the profunda, not knowing at that time whether the profunda or femoral, was the vessel injured. Two ligatures were applied, and the artery divided between them, in doing which, the surgeon instead of putting his knife under and cutting upwards, cut from above downwards, and cut the femoral vein more than half off. There was an alarming hemorrhage from the wound— a sponge was thurst into it without ceremony. About the sixth day we had violent hemorrhage: the gentlemen present, not attributing the accident to the right cause, tied the artery higher up, and sagely intimated, when a recurrence of the bleeding from the artery took place, two or three days afterwards, that we, who had applied the ligature for the operator, had not applied it tight enough. Examination after death, and before

any dissection was made, evinced that the limb supposed to be œdematous, was in reality, in a state of mortification. We watched this case throughout its progress, and we feel no hesitation in saying, that this patient lost his life in consequence of the division of the femoral vein. How far such a case would admit of remedy, is involved in some doubt, but in a similar one, we would endeavour to approximate the edges of the vessel, by means of a suture or two of fine leather, and if possible dress without compression.

We had the following case related to us, by a respectable physician—a man was shot high in the arm, it was easily ascertained that the humeral artery was divided. This vessel was tied, the hemorrhage ceased; a student was left with the patient, with tourniquet on the limb: some bleeding occurred in the night, the screw was applied, still it bled. The patient died in two or three days; and his death was ascribed to the great quantity of blood lost from the artery. Upon a review of the circumstances, we are led to believe, that this patient died from the loss of blood from the vein after the artery had greatly exhausted him, and had he not died from that cause, the limb would have been lost by the supervention of gangrene. And we give as our opinion, that if the vein is so far injured, in the lower edge of the axilla, as to prevent a restoration of its function by reunion, amputation should be performed.

General fever, by preventing the closure or obliteration of an artery to which a ligature is applied, may lead to secondary hemorrhage.—It will be more in place in our fourth division to speak more particularly of the influence of fever in the prevention of the closure, by inflammation, &c. of an artery. But we have thought proper briefly to notice the opinion of Dr. Jones, that, the force of the blood can never be such, as, merely by the action of the heart and arteries, to throw off a ligature. Mr. A. Burns is also of this opinion, but, indeed, so little ground is there for such an opinion, that authorities seem almost unnecessary. We wish, here also, to notice the proposal of Mr. Cline, Jun. for passing the ligature, by which an artery has been tied through its coats, outside of the ligature. This proposal has the approval of Dr. Smith, as editor of J. Bell's Surgery Abridged, but has been condemned, with some severity, by S. Cooper. For our own part, we have never seen any necessity for such an expedient; and believe that if a ligature is well applied, it will never slip off, and, therefore, to say the least of this proposal, it is wholly unnecessary.

Of local inflammation as a cause of secondary hemorrhage.—The practice of the older surgeons afforded many cases of this kind, in the treatment of aneurism, before the time of Mr. J. Hunter. They opened the aneurismal sac, and often tied the artery in a diseased state—a degree of irritation and unsoundness often existed, in the arterial coats, which led to dangerous inflammation of the vessel, while the admission of air to the contents of the sac, which could never be entirely removed, led to chemical changes, and these lead to dangerous inflammation. And in many cases of contused and lacerated wounds, the main artery of a limb has been tied without sufficient attention, as to the degree of injury which it had received. A vessel more or less injured, has often been surrounded by ligature, instead of dissecting along its course till the surgeon was fully satisfied he had a sound portion of the vessel. Having carefully attended to these circumstances, we have profited, for years past, in contused and lacerated wounds, by dissecting as much of the vessel as was necessary, and cutting away that part of it which we knew to have been injured, together with such parts of the rent cellular texture as was too much injured to maintain its vitality; by this procedure, we not only avoid the risk of secondary hemorrhage, but often succeed, in great part, in healing contused wounds by the first intention.

The destruction of the nervous power of a wounded limb, may lead to secondary hemorrhage.—This is a subject upon which, comparatively speaking, but little has been said. It is a matter of much importance, but, for practical purposes, it may suffice merely to point out the nature of injuries in which very serious harm has been done to the principal nerve of a limb, and leave the application in particular cases to the judgment of the practitioner: if, for instance, in a violent contusion and laceration of the fore-arm or wrist, there has been serious injury of one or both of the principal nerves; of the radal and ulnar arteries, or either of them moderately contused, or merely separated to some extent from their attachments, the reduction of vitality from loss of nervous influence may lead to hemorrhage, several days after the injury of the limb. It therefore becomes a matter of great consequence in some cases to ascertain the amount of injury done to the nerve, or nerves, before we make up our opinion as to the chances of saving the limb. If both the nerves are much contused or cut off, in wounds of the wrist, and the injury is otherwise great, it would probably, in some instances, be the safest to amputate at once, more especially if the patient

was at a considerable distance from surgical aid. Under more favourable circumstances, there would probably be but little danger in waiting till we see that the limb cannot be sustained; taking care if there be any risk of sloughing of the artery, to apprise the patient and attendants of the danger of hemorrhage, and provide for them the means necessary for arresting it. We would also augur unfavourably if there was any extensive injury of the axillary plexus, of the posterior crural nerve, &c. As wounds of this kind can only be properly managed, by a judicious application of measures, and as much must be left to the judgment of the surgeon, in each individual case, we shall close this article by the relation of an interesting, but unfortunate case.

A coloured man had his wrist much lacerated, by the machinery of a mill, but not so much as to do any material injury to the bones of the carpus; there was no bleeding to justify the least suspicion of either of the great arteries being opened--they could not be discovered to be injured. A physician of reputation united with us in the opinion, that an attempt should be made to save the limb. Several days after the injury violent hemorrhage came on. When we came to examine into the state of the hand, the tumefaction and want of sensibility were such as to justify the belief, that the nerves were destroyed. Having advised the necessary precaution in the event of hemorrhage occurring, the patient was saved from immediate death, by the application of the tourniquet; but this becoming painful, the patient prevailed on the ignorant attendants, that were left with him, to unscrew the tourniquet; and not taking the precaution to remove the blood, and bloody cloths, from about the arm, the patient was suffered to bleed to death, while his ignorant friends thought all was secure. In a word, in violent injuries, the state of the principal nerves claim a particular share of attention, and our opinion as to the necessity of amputation will depend upon the question, how much are the principal nerves injured? Simple division of a nerve might admit of reunion, other circumstances being favourable. The interesting experiments of Mr. Haighton support such expectation. Of these some account may be seen in A. Burn's work on Surgical Anatomy. We do not feel prepared to decide on the opinion of Mr. Abernethy, that where a nerve is divided the parts dependent upon it will be supported by anastomosing branches, but the experiments of Dr. Haighton seem to overturn such an opinion.

Diseased state of arteries may lead to secondary hemor-

rhage.—J. Bell, Abridgment of Surgery, (49,) has these important remarks; "the bursting of an artery after it seems securely tied, arises sometimes from the unfavourable state of the artery, and its being incapable of adhesion; but much more frequently does it happen from that process which should terminate in adhesion of the artery, proceeding to inflammation and ulceration of the arterial coats."—"We are sensible towards the decline of life, of changes in the large arterial trunks plainly unfavourable to our operations." He says further, speaking of the larger arteries in advanced life, "the whole tube is but little disposed to pass through those changes which are familiar to other soft parts. They do not inflame, adhere, thicken, and obliterate, as sound parts do. In dissecting an aged subject we always find the arteries less pliant, sometimes they are contracted, sometimes enlarged; they become white, and their coats thickened, and less connected with each other; separate like the coats of an onion into ten or twelve, or innumerable lamellæ; they are at the same time brittle and fragile, and have a crisped feel."

We are well persuaded, from actual experience, of the truth of the above facts. And we believe they are all decidedly in favour of the preference which we claim for the animal ligature. That the arteries of the aged are indisposed to heal we are well convinced, and, therefore, believe that by using a ligature which will cut the coats, neither in the art of tying, nor subsequently by attrition, we shall avoid much risk of suppurative inflammation, by which the sides of the vessel would be prevented from uniting. There is a case in point extremely interesting, in the Surgical Essays of Cooper and Travers. This was a patient aged 80, in whom the femoral artery was tied by Mr. Cooper, with a ligature of catgut steeped in water, which was cut close, and the wound was healed on the fourth day, and must therefore have healed by the first intention. In many cases, we believe, the catgut would answer as well as the buckskin, but we are confident the flattish ligature holds best; and is most convenient. It may be proper to mention, that this case occurred in 1817, three years after Dr. Physick's use of the leather ligature.

It is an opinion pretty generally received, that lacerated arteries are not disposed to bleed, and we know that even the humeral artery may be torn out, and no hemorrhage follow, as in the case recorded by Cheselden, and several similar since. We must not, however, place too much reliance on these cases. Where a limb may have been torn off, or an artery violently contused, so as to prevent its giving out its

blood, the utmost vigilance should be observed until we are satisfied the vessel may have had time to heal. We cannot easily know the extent of injury done to the artery; and its not bleeding, may be owing to a coagulum shutting up the vessel, or its not bleeding may be owing to the reduced state of the circulation, and in the first case when the coagulum dissolves, and in the second when some degree of re-action comes on, the wounded vessel may bleed violently. Every experienced surgeon must have seen cases in which arteries of considerable size will bleed time after time, so as to endanger the life of the patient, and stop almost of its own accord before the surgeon can reach the patient.

We have seen an interesting case of this kind: a middle aged man, apparently healthy, was wounded in the calf of the leg by a musket ball, he bled violently day after day; and it as often stopped suddenly. A consultation of surgeons and physicians agreed, that the interosseal artery was wounded; some talked of amputation, others of taking up the artery, but they had no precedent for such an operation; a third party prevailed in a proposal of stopping the wound by a plug of sponge. This would promise success for a few days, then a renewal of the bleeding would come on, and require a renewal of the sponge. Thus was the fatal tendency averted from day to day, till tetanus overtook the patient already worn out with pain and loss of blood; and he soon fell a victim to the wound, without having an effort made to save him, by so simple an operation as that for tying the interosseal artery of the leg.

The following interesting facts seem entitled to a place in the present article. C. Bell, (382,) tells us, that blood will coagulate in an artery much injured, or in which the coats have lost their vitality. (383) Torn arteries will not bleed, although the mouth of the torn artery remains open. Abernethy, vol. 1, (149,) says, "in examining patients who died after amputation, I have frequently seen the sides of the artery unclosed, even though the ligatures have fallen off from them." J. Bell's Abridgment of Surgery, (44,) "let us suppose the artery to be so small as just to spirt out its blood, the stream of blood gradually lessens, because the artery is emptied, and the resistance to the arterial action taken away, the stimulus being every moment lessened, acts less powerfully, and the blood being no longer solicited or urged on by the arterial contractions, *forsakes the open artery* and moves along the neighbouring branches." This is quite in accordance with the experience of J. Hunter.

It is an ascertained fact, that in mortification of a limb, the blood coagulates some distance above the sloughing portion of the arteries, and thus is the patient saved from hemorrhage; and when the disease is checked, the vessel will be obliterated by inflammation, and contraction of its coats. These cases are important, as serving to show the resources of nature in guarding, often, in so striking a manner, from the usual dangers of hemorrhage. But we must be careful not to make a wrong application of this circumstance, and rely upon nature's doing too much, or more than our experience will warrant. If we see nature now and then, competent to the task of healing torn, contused, or cut arteries, we must not lose sight of the fact, that in a great majority of cases, of injuries of arteries, they will sooner or later bleed and lead to danger. As a general rule then, we should examine every wound with the greatest care; and if a wounded artery can be got at, without putting the patient to too much pain, we should under all circumstances, prefer tying the vessel. If we cannot tie it, every precaution must be taken to guard against surprise from hemorrhage.

Pathology of blood.—This is a subject, upon which little can be said, in the present imperfect state of our physiological and pathological knowledge. Indeed, it may be that we might omit a single remark, under this head, without leaving this paper less useful; and we believe most of our readers are disposed to treat as chimerical, every thing which is said of the fluids of the animal system. For our own part, while we readily admit our ignorance of the actual nature or properties of the blood, in its state of health, or greatest perfection, and also in its state of disease, we feel equally confident, nay, we would say the thing is self-evident, that the blood is subject to deterioration, and in various degrees. We believe it may tolerably subserve the purposes of the system, and yet be so far incomplete in its composition, as to interfere with the healing of wounds, by the first intention. We have elsewhere expressed as our opinion, that the structure of brute animals is less perfect than that of man; and hence it is, that wounds in brutes do not heal as well by the first intention as in man. There is something pleasing in the thought, that man, although subject to death and corruption, yet, in his physical structure, is not entirely formed of brute matter. Now it is well known, that for the purposes of transfusion of blood, the blood of brutes will not answer for man. And it has been ascertained, by microscopic observation, that the globules of different animals differ in their organization, for we hesitate not to use

the term. Then, if we find the blood of one species of animal will not suit for the purpose of transfusion into another, and that that of each species has its peculiarities of globules —that the blood in man is subject to changes in health and disease, and varies, moreover, in its properties in the periods of childhood, adultness and old age, is there not abundant reason for supposing that the surgeon, in the closing of arteries by artificial, aided by natural means, may find his efforts more or less interrupted by a diseased state of the blood? Every one will admit the fact of the fluids becoming more acrimonious in old age, and, therefore, we may not only find this an obstacle in the way of healing arteries, but since we know that the solid structures of the body are subject to premature old age, or changes equivalent to it, so may we reasonably suppose that the blood too, may reach its state of decline before the usual period. If this does not afford us much practical instruction, it should, at least, teach us humility and caution in prognostication, while it stimulates us to double exertion to leave no point unexamined. We hope to prove, in our fourth division, that much may be done to aid nature in repairing accidental breaches in our bodies, by measures whose immediate influence is acknowledged to be over the sanguiferous system. Boyer, (125,) speaking of hemorrhage, tells us, that he has found hemorrhage sometimes unmanageable in depraved habits, particularly the scorbutic. Of the correctives for this habit, we have already taken notice on general principles.

Of the means by which traumatic hemorrhage is to be arrested.

By ligature in cases of amputation.—The ancients were not entirely ignorant of the use of ligatures, since mention has been made of them by Hippocrates, Celsus, and Galen; but we cannot possibly admit, that while surgeons remained ignorant of the true circulation of the blood, that they could have made a proper application of the ligature; and hence their preference for the actual cautery, stiptics, &c. The high merit of introducing the ligature into practice, in a state which, as regards the method, has been but little improved, is due to Ambrose Paré. Notwithstanding the very general use of the needle after his time, down almost to the present, he clearly pointed out the method of using the forceps and ligature to the arteries; still, if he had offered this free from the advice of enclosing much or little of the flesh, according to circumstances, it might not so easily have been misapplied, and nothing would have been left to his successors.

However clearly we may have ascertained the superiority of the method by the tenaculum, and tying the artery free from connexions, still the method of including a considerable portion of the surrounding parts in the ligature, was sufficiently successful to afford, alone, a complete refutation of the notion of Dr. Jones, that we must cut the arterial coats, to ensure their adhesion.

We are now prepared to speak of the application of the ligature in cases of amputation. It has been acknowledged, by all surgeons, that common ligatures of silk or flax, act as extraneous and irritating bodies in wounds, and hence the practice of cutting off one end. This was attempted to be improved by the fine ligature, by Mr. Lawrence, &c.; but though we admit this was an improvement upon the old practice, yet it has not always succeeded; and the fact of its often cutting its way out, is a proof of its liability to produce attrition and irritation. We have used the animal ligature in a considerable number of amputations of the limbs, twice in both limbs of the same patients, and in amputations of the mamma. In the latter cases we have, with few exceptions, healed the wounds by the first intention, in fourteen or fifteen days; and in the worst cases, we have never seen irritation from a ligature, and never more than slight suppurations, which delayed the healing a few days. In cases of other amputations, we have never seen any thing of our ligatures; have never had one to slip off; and the stumps heal sooner than when common ligatures are used. In amputations of the limbs, we can always avail ourselves of the tourniquet, or something which acts upon the same principle, i. e. pressure upon the vessel, as practised by Hennin, and others; and we are, in this way, enabled to stop the vessel from bleeding, as long as may be necessary; and, therefore, there can be no apology for tying the arteries insecurely. We have seen a branch of the anterior crural nerve, more than once attached to the femoral artery, but found no difficulty in separating it with a scalpel, while we held the artery on a tenaculum. We have had occasion, in depraved habits, to tie some of the larger veins; we have seen a number of veins pour out blood freely in such habits; and have arrested it, by using the crooked needle, by means of which we included in an animal ligature, a considerable portion of the surrounding structure; and our experience warrants us in saying, that no injury will arise from using an animal ligature, in that way, where it may be found necessary. We have never had occasion to open a stump, on account of hemorrhage; such a thing, we believe, never hav-

ing occurred where an animal ligature was well applied; and we have never known one applied otherwise. The other circumstance, to which we alluded, is that of exposing cut surfaces freely to the atmospheric air, and never being in too much hurry in closing surgical wounds. For this we are indebted to J. Bell; we not only wait till the blood coagulates well over the face of the stump, but until there is a degree of action restored to the arterial system; about half an hour may usually be considered the proper time. Of ossification of arteries, and other diseases of the vessel, we have already treated, under the several heads, in our observations upon pathology.

C. Bell, (387,) advises, in cases of secondary hemorrhage in amputation, to cut down, and tie the vessel. We believe that, where the animal ligature is used, this operation can never be necessary. If the artery be not too much diseased to admit the application of the ligature at first, there will be no occasion for re-application. If the artery is found too much diseased, we have already said, the artery should be plugged. Boyer, vol. I. (139) relates a case of amputation of a finger, in a scorbutic habit, in which secondary hemorrhage gave much trouble, and only yielded eventually to anti-scorbutics.

We have, in a former section, given as our opinion, in speaking of the physiology of the arteries, that those of the penis and mamma were disposed to take on, when diseased, something like an erectile action, and therefore requiring special care when wounded. The following case, not precisely calculated to support such an opinion, is nevertheless highly interesting. In amputating a scirrhous mamma, a short time ago, we divided the largest artery we have ever seen in those parts; it was easily secured, but there being some fears that the parts about it were not entirely sound, a portion was removed, in doing which the ligature was cut away, and about half an inch of the artery was exposed, large as the internal mammary, and quite tortuous. Not a drop of blood was seen to escape from the wound opposite to the mouth of the vessel. After tying all the arteries which were evident to bleed at all, the wound was dressed, and the patient left apparently comfortable; in about half an hour, it was discovered that a serious hemorrhage existed. The wound was opened, and after much search, and removal of considerable quantities of coagula, it was discovered that the lower portion of the artery just noticed was bleeding in a retrograde direction, as freely as it could possibly have done in its natural course. This

case should be a warning to surgeons, not to leave such patients without a person being present, capable of affording relief by pressure, till he could be sent for. We shall close the present article, by reminding the reader of what we have said respecting the situation in which the arteries may be found in a stump, immediately after amputation. They will sometimes be seen standing open mouthed, and not at all retracted, for the reason already pointed out, (article physiology of the arteries.) In other cases, one or more may have retracted in among the cellular structure, so as to be concealed from view; we unscrew the tourniquet, in such cases, and are thus directed to the vessel. There can be no occasion for hurry; and if the surgeon is prepared, in his mind, for such an occurrence, he will not suffer himself to be agitated, because persons looking on may suppose he is awkward in getting the vessel. If he is conscious the fault is not in himself, he will be collected, and do his work securely.

Of the application of the ligature in cases of wounds.— In the first lines of S. Cooper, (152,) reference is made to the experiments of Jones and Blecard, which were intended to prove, that wounds of small dimensions, in large arteries, will heal without obliteration of the vessels. Mr. Cooper has, however, very properly remarked, that it is a point settled by dissections, that no artery wounded, and healed under pressure, will remain pervious. These experiments, therefore, afford nothing applicable to practice. To trust a wounded artery to nature, or to attempt the application of sutures with a view of closing such wounds, will never be justifiable in the human body; and to attempt to close such wounds by compression, if the injured vessel be of considerable size, is to incur the risk of disappointment and danger, which may be avoided by the application of the ligature. To attempt the obliteration of the vessel by the graduated compress, with the aid of the tourniquet, applied in any form, is alike awkward, disadvantageous, and never free from uncertainty. We are, therefore, led to object to the proposal of Mr. Cooper, of healing such wounds by compression, as a matter of choice. If an artery of considerable size be wounded, the best practice is to cut down and tie the vessel. We readily admit, that in children, and, even in adults, we may often succeed in healing arteries of the feet, lower part of the fore-arm, &c. by pressure; but some degree of risk attends every such case, unless the attendants are intelligent, and the surgeon near at hand. In a word, we would in all cases, notwithstanding the experiments, upon small

wounds in arteries, just alluded to, treat every such case as we would the same vessels divided transversely. In either case, if circumstances are favourable, we may make an attempt to obliterate the vessel by compression; but if the vessel be large, or the situation of things, upon the whole, unfavourable to compression, the vessel should be tied at once; since it has happened, again, and again, that vessels restrained for several days, by compression, have bled and continued to bleed day after day, till they were tied. We should not lose sight, here, of the observations of Mr. C. Bell; Operative Surgery, vol. 2, (415 and 416,) respecting the disadvantage of applying compression where we are compelled to obliterate an artery of importance. To lose the use of an arterial trunk is enough, but to have the circulation materially interrupted, by bandages around such a limb, must materially endanger the loss of it, for want of the necessary supply of blood. In gun-shot wounds, we cannot always be able to say, whether a particular artery be wounded or not. If there be profuse bleeding, and the ball seems to have gone in the direction of an artery, we must proceed to cut down to the vessel, where wounded, or at some more convenient part above the wound. In these wounds two questions may arise: If there be hemorrhage, must the vessel be tied, or may we trust to compression? secondly, if there be no hemorrhage, but contusion immediately adjacent to an artery of considerable size, what is to be done? In answer to the first, A. Cooper, in his lectures, (115,) says, "an artery divided by a gun-shot wound does not bleed, as in other injuries; and the reason is, that, a fringe of cellular membrane is formed at its end, and, within this a coagulum forms."—"An officer who had been shot through the leg appeared to be doing so well, as to be directed by his surgeon to walk about, and without the tourniquet, and from the increased exertion hemorrhage came on, and proved fatal before any one could assist him." We merely cite this case from among the great number to be found in our books, of the same kind, with a view of putting the young surgeon on his guard; and consider it a duty, to contrast with it, the opinion of C. Bell. Speaking of a campaign at Corunna, he says, "It is a singular fact, that of all the hundreds of wounds, of balls in these wards, with sloughs hanging from the wounds, there is no hemorrhagy."—"My inquiries were suspended. I found no instance of secondary hemorrhagy. I began to think it was equally incorrect to say that every artery touched with a ball must slough and bleed, as it is to say, that every bone

touched by a ball must exfoliate." We believe no well informed surgeon would expect hemorrhage in every case of gun-shot wound near an artery. This concluding clause of Mr. Bell, is, therefore, not altogether candid. But he has very properly taken care to advise the younger members of the profession, "to take the precaution, recommended by so many, in regard to secondary hemorrhage from gun-shot wounds." We would only say, that any unnecessary anxiety about a patient for whom we have taken every precaution, would be a weakness; to do less than this for him, might become a crime. But how much better is it, that we, in hundreds of instances, unnecessarily provide against what may suddenly endanger the life of a patient, than indulge in hopes, growing out of the fact, that the risk is moderate, and thereby lose a patient: the loss of a single patient, under such circumstances, would embitter the life of a good man for years. We readily admit with Mr. C. Bell, that there may be copious bleeding from gun-shot wounds, where no large artery is wounded, but let this not lead us into security. If there is reasonable presumption of a wound or contusion of an artery, put on the tourniquet; keep the patient quiet, and let him be watched, that the screw may be tightened, if necessary. If there be repeated bleedings, cut down to the artery, and tie both ends—if it be partially divided, apply two ligatures, and divide the artery between; taking care to use the precaution, pointed out by Mr. A. Cooper, to tie about an inch above the part of the artery injured, on the upper side, if it can be done: below, it is of less moment, but, where easily done, it should be done also, and the portion of artery between, so far as visibly contused, should be dissected out. Where there is evidence, as evinced by hemorrhage in such wounds, as well as in incised wounds, we should cut down to the artery, and tie the vessel above and below the wound. We had a patient who had his fore-arm torn by a large stone falling on it; the radial artery was denuded, and appeared much contused, in fully an inch and an half of its length. The sheath was split up about three-fourths of an inch, and an animal ligature applied to the sound artery; and the lower portion managed in the same manner. All that portion of the artery visibly injured was removed, as also, all the contused cellular tissue. The wound, which was nearly in the longitudinal direction of the arm, was now carefully closed, by adhesive plaster. Nothing was ever seen of the ligatures, and nearly the whole of the wound healed by the first intention—a little pus issuing, for a few days, from the lower an-

gle of the wound, the outer or superficial part of which closed before the first dressing. We also saw a case where, by the bursting of a gun, a large wound was made in the fore-arm by some part of the fragments. The artery was torn off, and bled freely. The patient, however, by grasping the parts firmly, was enabled by the favourable course of the wound, nearly in the direction of the radius, but somewhat spiral, to prevent immediate death. A physician of experience in the neighbourhood applied, shortly afterwards, a tourniquet. When we arrived, we dissected the artery clean, till satisfied of having a sound part—an animal ligature being applied, and the tourniquet loosened, the lower portion, together with two anomalous branches, about half an inch apart, and the first as far from the radial, bled furiously. These were tied with the animal ligature, the injured artery and cellular structure were removed by the scalpel: the parts carefully brought together, by adhesive plaster, healed, in much the greater part, by the first intention, although the wound was contused and lacerated; so that we had no expectations of such a result. The ligatures were never seen, nor was there any pus from the wound. We have mentioned an interesting case of sloughing of the femoral artery from gun-shot wound, in speaking of wounds of the veins. Boyer, in his Surgery, vol. 1, (183,) says, arteries often bleed two or three days after gun-shot wounds.

C. Bell, Operative Surgery, (402,) says, that when an artery is wounded, in the foot, or hand, if a surgeon be called immediately, he can frequently restrain it altogether; but on the contrary, if the blood has once been infused among the surrounding structures, we will often have to tie the vessel, or a larger in the fore-arm or ankle.

Again: to the query, do all such wounds require ligatures? he says, "certainly not; they will often stop bleeding, and can be suppressed." These cases like all others in surgery, call for the discriminating judgment of a surgeon. We believe that, in general, if there be some contusion and a divided artery, if called early, compression will succeed, but if there has been extravasation, or, if after dressing carefully, there be any considerable hemorrhage, we had better tie the vessel in the wrist or ankle. The remarks of J. Bell, respecting the thickening of the cellular structure, and of the stuffing of coagula between the different layers of aponeurotic structure, are highly important, and will be found to be strictly true; and, therefore, unless we can see the vessel, or think proper to operate before there is much injection of

blood, we should always take up the arteries just above the wrist or ankle. A leading consideration in these cases is, whether the artery is much contused and partially wounded, or wholly divided? Wherever it can be easily done, prefer tying the vessel, if it bleed freely. If the circumstances are favourable for compression, and the vessel is divided so that it may retract, try it, with proper precaution; if the vessel is punctured, or partially divided, with a contused wound, you must always apply the ligature.

We were called to a little female patient who had cut her wrist, severely, by falling on a glass tumbler. There had been so profuse a bleeding, that no doubt was left of the ulnar artery being wounded, we carefully ascertained by feeling, that the artery pulsated just above the wound, but not in it, and, there was now no bleeding. We felt satisfied the artery was divided transversely. We therefore agreed to the proposal of the family physician to try compression, which we would not have done had we not been satisfied of the complete division of the artery. The plan adopted succeeded well.

As a general rule, it may be said, that where we tie an artery that is wounded, we should apply two ligatures; but we have no hesitation in saying, that if the animal ligature be used, there is no advantage whatever in dividing the vessel between the ligatures. If there be severe contusion, which is likely to lead to sloughing, we should treat the contused part as we would those around; if much bruised, cut them out. It has, however, been correctly said by J. Bell, speaking of wounds of the hands or feet, "I never saw it necessary to tie the lower end of the artery, and I think it important to mention this as an exception to the common rule laid down of tying both ends of it." Notwithstanding the alarm in cases of wounds of the palmar or plantar arch, and the danger which often attends the method of compression in these cases, we readily agree with Mr. Bell, that, however obstinate the bleeding from the upper end, and however certain we are that bleeding will take place from both ends, while we trust to compression, yet, after securing the direct current by tying the vessel, the retrograde current through the arch will easily be controlled by compression. We saw a case, in which a wound of the plantar arch, near the heal, bled from day to day, for two weeks, compression seemed only to serve to drive the blood more furiously among the aponeurotic structures, and the pain became intolerable. We tied the posterior tibial. Upon removing the coagula which were wedged with incredi-

ble tightness in the sole of the foot, a pretty free bleeding continued, from the anterior portion of the vessel; a very moderate degree of compression served to control the bleeding, which did not return.

J. Bell, (95,) reports an interesting case, from Ducamp, of a wound in the arm. He says, there is "difficulty in finding the exact place where an artery has been obliquely wounded, by the slanting stab of a knife, or sword, or the course of a ball,"—"the obliquity of such a wound acts like a valve upon the artery; there is but little blood poured out, and no remarkable tumour is formed." In the case alluded to, "the knife entered the outer edge of the biceps, and touched the brachial artery." The patient fainted with loss of blood: an ignorant student soon after bled him, and bound up the arm. This patient bled from time to time, and at length the artery was laid bare, and a ligature of reserve put under the artery but not tied. The patient was suffered to bleed, from day to day, till he was exhausted before this ligature was tied, which was no sooner done than it was discovered that the ligature was below the wound in the artery; for now the blood gushed forth, but the patient was too far exhausted to bear further sufferings. Mr. C. Bell has very properly advised in such cases to take hold of the artery, and close its sides, if the wound be above, the vessel will bleed. Where we meet these oblique stabs, attended with bleeding so profuse, and in a direction to justify the belief of an artery being wounded, a large probe or straight catheter should be introduced; and if we find nothing in the course of the wound, it being superficial, to forbid laying open the wound, in all its extent, a director may be passed in, and the bistoury used in its groove, to effect our purpose. If the intermediate parts between the external wound, and that of the artery do not admit of this, a strong probe may be introduced, and by feeling where its point is arrested, we may cut down upon it, and there expose the artery.

J. Bell, (79,) says, "if there be a wound of the great arteries in the back of the hip, in the groin, in the arm pit, we cannot command the blood easily; we are not sure of clapping our finger down upon the artery at the very point where it is wounded. We are afraid the patient should die with one single gush of blood; we therefore close the narrow wound, put its lips together," &c., "the lips adhere, and then we have an aneurism which we can look upon composedly." Our author goes on to advise us, after the aneurism has formed, to lay it open, and tie the vessel in the sac, and supports his opinion by an imposing case. We think proper here to differ, in some measure, from Mr. Bell. In the first place, we

have no certainty, that the blood in such cases will not be extensively diffused among the muscles. The parts must, by their separation, and the presence of the blood, acting as an extraneous body, become spoiled, the artery itself likely to become diseased in its coats, &c. These objections, in our opinion, overbalance any advantages supposed to arise out of the delay. We think it may be laid down as a pretty general rule of practice, that if an artery is wounded, which we intend to tie, it should be done as soon as practicable after the injury; and if we are not called till after a quantity of blood is extravasated, we would, wherever practicable, apply a ligature to a sound part of the artery above, and not open the sac: at all events, not before the artery, (supposed to be tied above,) is healed. Mr. Bell was truly fortunate in saving the patient, whose case is brought to support the opinion just cited. To say nothing of the uncertainty of finding the artery, in such a cavity, before the patient would bleed to death, and the uncertainty of tying the artery, where free from disease; formidable objections to such an operation, grow out of the fact of a horrible wound having been made, which required seven months to heal it, besides the risk and suffering necessarily growing out of the admission of the air to so large a cavity, filled with materials liable to be acted on by chemical agents. Since we know that the external iliac has frequently been taken up with success, and even the internal, from which the gluteal, ischiatic, and pudic arteries, are given off, we should greatly prefer tying the latter vessel, in such a case as Mr. Bell relates, and believe there would be less suffering, less danger, and a much more speedy cure. We cannot conceive of any situation in which an artery may be wounded, that is at all accessible to the surgeon, in which we should adopt the advice of Mr. Bell. just alluded to. We can in all cases arrest the bleeding by the tuorniquet till we cut down and tie the vessel, or we can thrust in a finger, and compress the vessel where wounded; and if we cannot avail ourselves of this, by a little enlargement of the wound, we can force down a small piece of sponge, and confine it by the hand, or a compress and bandage, till we have time to cut down upon the artery, and tie it, as in cases of aneurism.

Perhaps no well informed surgeon would doubt the fact, that persons have died under the care of good surgeons, from hemorrhage in cases of lithotomy. And we believe it is equally clear, that in some of the fatal or dangerous cases, the bleeding was from vessels seated about the neck of the bladder. Surgeons are generally aware too of the liability of patients to

hemorrhage from operations about the anus or rectum. In all these cases, except the wound of the pudic artery, the danger arises from the same cause, that is, the flaccidity of parts in which these vessels are seated. We have not room here, to enter into a full discussion of the subject, and shall, therefore content ourselves with the relation of a few cases, to show our views and practice. We have, on several occasions, had alarming hemorrhage, after cutting into the neck of the bladder. We have always succeeded pretty easily in controlling such hemorrhage, by pressing in a piece of sponge, taking care first to secure a free passage for the urine, by introducing a tube through the urethra, and into the bladder. Over the sponge we laid a compress, and secured the whole by a T bandage. A little blood sometimes passes into the bladder, but it will seldom coagulate, the urine serving to prevent it. By tepid water thrown in, and drawing it out again, by means of the syringe applied to the tube in the urethra, we can relieve the bladder from injury, from the presence of the blood. In cases where the pudic artery is wounded, we must avail ourselves of our anatomy, cut down for it, and secure it by means of Dr. Physick's forceps, and a suture needle somewhat more curved than usual.

We have twice had cause of alarm in operations upon the rectum, for stricture in one case, and extensive fistulous ulcers in the other. In the one case, the sponge succeeded in arresting the hemorrhage; in the other, such was the uncontrollable action of the parts to expel the sponge, that we were compelled to depend upon pressure with the finger. We were the more disposed to adopt this mode in the case alluded to, on account of the patient having lost a dangerous quantity of blood, before we were called on account of the bleeding. By compressing the vessels with the finger in ano, about three hours, the bleeding was completely overcome. The interesting case of Mr. Gooch, in which pressure was applied in this way for eight days, shows to what advantage this form of compression may, under particular circumstances be applied. In a word, then, wherever vessels bleed dangerously on account of their loose connections, which deprived them of the advantages of retraction, and the surgeon the opportunity of applying pressure by bandage, the vessels are but of moderate size, and therefore, although they may sometimes be numerous, we may venture to assert, from our own experience that we can by a succession of assistants, control the bleeding, by pressure with the finger, till the vessels are no longer disposed to bleed; and a knowledge of this increases our confidence in operations

upon flaccid and vascular parts; since we can be assured, that by means of the sponge, or pressure with the finger, all danger may be obviated.

Aneurism may become a cause of hemorrhage, and the wrong application of the ligature a cause of secondary hemorrhage.

Since aneurism may become a cause of hemorrhage, if the case is suffered to progress too far before a ligature is applied to the affected artery, it seems necessary briefly to notice this as a cause of hemorrhage; and to notice the particulars which should govern in the application of the ligature, in such cases, to prevent secondary hemorrhage. Our observations having already extended beyond what we had anticipated, we must be quite brief in the examination of the subject before us. A translation of Scarpa's observations, is to be found in the Philadelphia Journal of the Medical and Physical Sciences, No. 2, new series. We beg leave to recommend that paper, so peculiarly interesting, to every surgical reader. The introduction of the animal ligature has, however, so materially improved the treatment, in the operation of tying arteries, that it becomes necessary to object to the views and practice of Scarpa, so far as this circumstance relates to the subject.

Mr. J. Bell, Abridg. Surgery, (51,) "The diseased state of the arteries never can explain the difference, in the danger of the two operations, of aneurism and amputation. In amputation of the thigh, we tie the great femoral artery, we tie also the profunda and its muscular branches; we perform amputation daily, and we tie many great arteries in each amputation; each ligature is seen moved and raised up from the face of the stump, at every pulse of the artery; the stump remains open. These arteries continue for weeks to bear the whole force of the circulation unsupported, yet they seldom give way." Our author goes on further, in speaking of operations for aneurism, to say, it is an operation never performed, but by surgeons of the first eminence, and yet, perhaps, more died than have survived it. He then reminds of the fact, that Mr. Hunter lost some of his patients, and that Mr. Pott, in one case, was obliged to amputate on account of secondary hemorrhage; that in other instances, where patients were ultimately saved, they were endangered by secondary hemorrhage, &c. We are well aware of the truth of the remark of Dr. Smith, in a foot note on the above paragraph, that "the success of the operation has been much greater of late." Those failures have been very generally, perhaps universally attributed in greater part to detaching the artery too

much, and although we readily give our assent, in the general, to this opinion, yet, we must not forget the fact that Mr. Hunter himself became convinced of the importance of leaving the artery as much as possible in situ, and still cases of secondary hemorrhage continued to occur. Too much importance was, therefore, given to this part of the operation, though as a part of the measures necessary for insuring success, it should never be lost sight of by the surgeon.

The profession, having found that this circumstance was not sufficient of itself to ensure success, with a zeal commensurate with the importance of the subject, instituted, with much judgment and industry, experiments upon living animals. Among these, Dr. Jones did honour to the profession and himself; and by the introduction of smaller ligatures, applied with special care, he certainly much improved the practice, and rendered the dangers from secondary hemorrhage much less. He, however, instead of ascribing his greater success to the right cause, attributed it to the circumstance of his ligatures cutting the coats of the arteries. The imposing nature of his experiments, and the greater success attending his practice, was well calculated to carry conviction as to the truth of his theory; but if he and his followers had stopped to weigh the matter well, they must have seen a complete refutation of such an opinion in the practice of every day; arteries were healed when torn off, when cut out by mortification, when included in a mass of flesh surrounded by ligature. In addition to these effects from direct violence, the vessels were seen obliterated by the pressure of tumours, aneurismal and others, &c. Besides, although the smaller ligatures improved the practice, still they did not wholly remove the risk of secondary hemorrhage. This disappointment led to other interesting experiments, by Scarpa, Crampton, &c., some of whom not only were convinced of the wrong conclusions of Dr. Jones in this particular, but that cutting the coats was in many instances prejudicial. Their various experiments, and the results are too generally known to require an expose of them here. Let it suffice to say, that their theory was correct, but their practice wrong, or inefficient. They aimed at obliterating arteries, without cutting their coats, by which they said the vessel was weakened, and too much inflammation liable to supervene, by which union by the first intention might be prevented or disturbed, before the parts were sufficiently united; i. e. by superinducing suppurative inflammation, the vessel might be prevented from healing by the first intention. They discovered that they

could only effect their purpose by large or flattish ligatures; these would, after a certain period, become sources of every mischief; and, therefore, they were removed as soon as the vessel was supposed to be in a state to bear it, (to wit,) about the fourth day. This, then, is the extent of Scarpa's views, and we have already said, that we consider his method superior to that of Jones in the larger arteries.

We are not able precisely to state the views of Dr. Physick upon this subject, but to him is certainly due the merit of introducing the animal ligature into practice. We will endeavour to do the Dr. justice; if we fail in it, it shall not be wilful. We have been creditably informed that Dr. Dorsey, acting under the auspices of Dr. Physick, was in the habit of cutting his ligatures from chamois leather, and rolling them on a slab to make them round. This, to us, is a clear proof that Dr. Dorsey at least, did not coincide with Scarpa in opinion, but with Jones. And we know experimentally, whatever may have been the theory of those gentlemen, that a leather ligature rendered hard by rolling, or close drawing through our nails, is liable, if applied tight, to cut the inner coats. This practice therefore was not suited to the views of Scarpa, Crampton, &c.

If we are right in the opinion which we have just expressed, Dr. Physick is entitled to the credit of bringing into use the best ligature, as to the material, but here his claim is at an end. The following results seem to have been attained—The experiments of Hunter and Jones, show the importance of the vasa vasorum, and consequently the advantage of leaving arteries to be tied, as much as possible, in situ. Scarpa and his associates in opinion, have shown the superiority of the method of applying ligatures which will not cut, nor bruise, the coats of the artery; and the advantage of removing them before the suppurative inflammation is produced by their presence. Dr. Physick and Sir Astley Cooper, have shown the advantages of using a substance which will serve as a ligature till the artery is obliterated, and be speedily afterwards in the power of the absorbents, so that they will remove it. Now, in the practice of Hunter and Jones, although they were aware of the importance of preserving the vasa vasorum, yet, we see them cutting off the true vasa vasorum, and relying chiefly on the vessels subsidiary to them. The disadvantages of this method, then is, that they weaken the arterial coats, destroy the circulation in the true vasa vasorum, and thus produce too much inflammation. This method nevertheless will succeed very well in vessels of mo-

derate size, but in the larger arteries it will not, because large vessels when tied, fall into puckers, the coats are cut irregularly, the cellular coat is too weak to confine the tissue arteriel; and consequently its elastic power will raise some of the cut folds above the ligature, drawing the yielding external coat with it, the sides of the vessel between the ligature and the heart will not be in contact; no union will take place, and secondary hemorrhage will, sooner or later, follow the use of such a ligature. We have two cases in point, those of Dr. Mott, and Dr. Græffe, upon the arteria innominata. In the practice of Scarpa and others, pointed out above, all the objections to that of Jones are avoided, hence the preference of the former over the latter. But strong objections grow out of the circumstance that we cannot heal the wound by the first intention. On the fourth day, we are to cut the ligature and remove it. This part of Scarpa's practice is exceedingly objectionable, because the wound must be kept open, and to effect this, we must often introduce some extraneous body into the wound to prevent its healing before the fourth day: and no dexterity of the surgeon, nor ingenuity in the construction of an instrument to effect this purpose can be devised, which will obviate considerable pain to the patient, and perplexity and apprehension to the surgeon in cutting the ligature. We have cut for the femoral artery in a patient in whom there was fully one inch of fat over the artery; in such, and similar cases, there would be much difficulty in cutting a ligature.

A decided preference must be allowed to the method of Physick and Cooper, as it involves facts and principles which free it from all the objections to which the methods of Jones and Scarpa are liable. But notwithstanding the facts and principles afforded by these celebrated men, they have left the subject susceptible of improvement; and to point out, and establish upon substantial and solid ground, some very material improvement in this department of surgery, has been the chief aim and design of this essay.

We will now proceed to point out our own views, and endeavour to support them by experiment.

1. We believe that if an artery is sufficiently healthy, to admit of its obliteration by adhesion of its sides, it is best done by a ligature which will neither cut its coats, nor strangulate except in parts, the true vasa vasorum, so that the continuity of the vessel shall not be destroyed, although we obliterate its caliber.

2. We believe that if an animal ligature of the proper

kind, be properly applied, the vessel will be obliterated, the wound may be healed by the first intention, and the ligature will not cause suppurative inflammation; but in due time, being dissoluble, the whole will be removed by the absorbents. There will be no breach of continuity in the artery, the materia arteriarum will be removed, and the vessel which during the state of inflammation and effusion of lymph, was converted into a cord, will pretty soon afterwards, be resolved into a flat string of white cellular structure.

EXPERIMENT I.—The carotid artery of a sheep was laid bare, and a common suture needle was passed through the artery, armed with a tapering ligature, the largest end of which was about three lines in width, and its thickness that of pretty thick buckskin. It was cautiously drawn through nearly to the largest end, both ends were then cut off, about three lines from the vessel. Not a drop of blood escaped. The animal was killed, twenty-two days after the experiment, in good health. The artery was greatly thickened in its coats, about an inch above and below the seton—No traces of a wound could be seen on one side of the artery; on the other there was a slight depression, in which lay a little yellowish matter, but which, on examination, proved to be a small part of the buckskin remaining within the vessel, and reduced to a pulp, by its mixture with a portion of lymph. The sides of the vessel were closely in contact, but not quite united, a flattish opening was seen, through which a very small probe might have been passed. There is no doubt, that this vessel which was thus gradually contracted in its caliber, would in a short time have been quite obliterated; and we are quite certain, no blood circulated through it, at the time the animal was killed.

EXPERIMENT II.—The jugular vein of a sheep was surrounded by an animal ligature, rendered firm by drawing it, after cut from the skin: at the end of six days the animal was killed, the ligature was still round the vessel, but upon scraping it gently with a knife, the skin was found as thin as tissue paper. The vein lay in puckers or plaits within, and there was no union of the coats under the ligature. Above the ligature the vessel was inflamed, and distended by a coagulum which adhered to its sides, and was about two inches long. The vein became thickened a little below the ligature, and soon assumed the size of the vein when distended with its blood, but was solid; when cut across it had the appearance of a transverse section of the human œsophagus, though traces of its caliber could be seen, yet it would

not admit the end of a small blow pipe—No pus in any part, but lymph still exuding—In short the vessel was completely obliterated, without union of its sides, but by thickening of the coats, as represented by Mr. Travers. It is evident here, there would have been no breach of continuity of the vein, and in a few days, the small remains of the ligature would have been removed.

EXPERIMENT III.—The left carotid of a dog, was laid bare and transfixed with a buckskin string, as in experiment first. The animal continued playful, and although there was considerable swelling of the throat, it eat well every day. This animal was killed on the twenty-fifth day after the operation. No traces of any violence remained in the parts till we came down upon the artery, which was slightly contracted, somewhat thickened, and hardened in its coats. Small red dots were seen here and there, within the artery, and on the side next the heart the inner coat was a little fuzzy. On either side of the vessel were seen two little yellow knobs, which proved to be the two ends of the ligature, in the state of yellow pulp. The knobs were completely enveloped in a strong membraneous capsule; the same capsule enclosing both knobs, and its fibres plainly seen crossing over the artery. This arrangement of the capsule seemed to have the effect of drawing the button-like knobs together, and was thus closing the vessel. If any circulation had remained, it must have been very little, as the artery was so contracted just where the string passed through it, as to be in contact with the inside coat of the artery. On the lower side where the coats were not cut, in splitting open the vessel to examine its interior, there was an actual adhesion of the leather to the inner coat. The string was softened into a pulpy consistence, and near the inside of the vessel was evidently covered with organized lymph, and extending along its edge on one side, some distance. When the string was gently pulled, one plainly saw a delicate membrane passing off from the artery, and ending on the string, in fringed extremities. This attachment of the vessel by a new formation, existed on both sides of the artery, and also, at its inner and outer sides. The capsule covering the knobs or ends of the string, was fully equal in strength to the outer coat of the artery, and therefore, there was no tendency to hemorrhage.

EXPERIMENT IV.—We tied the left carotid low down, in a sheep; at this part, the vessel lay deep; and a good deal of violence was done to the parts, before the artery was fully exposed. It was detached from its connexions

about an inch and a half. This animal was extremely wild, seemed unwell, and would not eat freely before operated on, scarcely at all afterwards. An animal ligature was applied tight. On the sixth day, this animal died; the parts were in a state of gangrene, and therefore little information is to be had from our examination. We remarked that the ligature was very tight, had cut the inner coats nearly all round; no adhesion had taken place. We cite this case to show that a *drawn* buckskin ligature will cut the inner coats, and to show that where the inflammmation runs too high for the production of lymph, the ligature will not readily dissolve.

Experiment V.—We exposed the jugular vein of a sheep, which was unusually large, say three-fourths of an inch in diameter. A tapering string of soft buckskin was passed through it, as in experiment I. At the end of six days, this animal, being the subject of experiment IV., died. On examination, the vein was affected with gangrenous inflammation, but had contracted at least two thirds of its size where transfixed. Some lymph surrounded the ligature, of a reddish colour, giving it much the appearance of a pulpy mass of flesh. The sides of the vein closely embraced the leather, and there was plainly to be seen, inside of the vein, membranous shreds passing off along the string. This vessel would doubtless have been obliterated, and no risk of hemorrhage existed.

Experiment VI.—We tied the jugular vein of a dog, with a drawn animal ligature. At the end of nine days the animal was killed, and the following appearances presented, the animal not having been sensibly injured in its health. The vein immediately below the ligature, though somewhat puckered, was healed by adhesion of the inner coat, according to the views of Mr. Hunter; above and below, for nearly an inch, the coats were greatly thickened. The inner coats were divided, and the two ends of the vein separated more than half an inch. There was no coagulum on either side of the ligature, nor any appearance of inflammation. About half an inch of the vessel, on both sides of the ligature, was greatly thickened. The ligature below the vein was completely removed, and a pulpy yellowish little button-like fragment of the ligature, remained on either side of the vessel, but detached from each other, and surrounded by a quantity of lymph. No appearance of pus. The parts around were not inflamed nor diseased. The appearances in this case exactly resembled the drawing in Mr. Traver's thirteenth plate.

Experiment VII.—The superficial femoral artery of a

sheep, a vessel about the size of a crow quill, was dissected up a small distance, and torn out. The animal did not seem to mind it; there was but two or three teaspoon-fuls of blood. We found this artery contracted much more than a dead artery, and it had a sort of writhing motion when handled, convincing us of its vitality. When examined after the death of the animal, the artery was filled by a coagulum about an inch long, and it was contracted and hard, like the vas deferens; the cellular tissue around it somewhat injected with blood, but not so much as to produce any pressure upon the artery.

EXPERIMENT VIII.—A number of experiments were made upon a dog, which had been bled to death the day before; ligatures of various kinds were applied, from just below the arch of the aorta to near the iliacs. And the following results were obtained.

1. Ligatures of four common coarse sewing threads, twisted slightly into a cord, would not cut the inner coats.

2. Ligatures of two threads applied tightly, not only cut the coats entirely around, leaving nothing but the thin outer coat, but by cutting the vasa vasorum, a small portion of blood escaped, and formed a red line, which appeared on removing the ligature.

3. Buckskin ligatures, of common size, *drawn through the nails of the thumb and forefinger*, cut the coats more or less.

4. A flattish ligature, *about two lines in width, not drawn through the nails*, but applied tight, neither cut nor bruised the inner coats.

5. Ligatures of buckskin, of common size, *not drawn, applied tightly*, did not cut the inner coats. But all kinds of the leather ligatures could be applied, with care, tight enough to bring the sides of the artery into contact, without cutting the coats. In every instance, the vessel was much puckered, but still the sides, in all cases, lay pretty well in contact, but where tied with the cutting ligature, certainly no better than when they did not cut. Indeed, we thought there was a preference, in this respect, in favour of the soft animal ligature loosely applied. We have practised these experiments upon the large arteries of human subjects, and find no difference, except that the coats of the human kind do not so readily give, under the animal ligature, and with any tolerable prudence in the application, they will not cut the inner coats at all.

We trust, we may now venture to assert, that we have arrived at the following important conclusions: 1. Hunter and Jones were right in advising us to take care of the vasa va-

sorum. 2. Scarpa, Crampton, Travers, &c. were right in objecting to cutting the *tissue arteriel.* They did not succeed, however, in devising the proper means of securing the intention of obliterating vessels without destroying their continuity. 3. Physick, and Cooper, by employing a ligature having the property of dissolubility, greatly improved our practice, whatever might have been their theory. Physick's ligatures, first used in 1814, have done much good. Cooper by tying, in 1817, the femoral artery of a man aged 80, and healing the wound by the fourth day, shows the superiority of this method over that of Scarpa.

We aim at a combination of the improvements of all these parties; we say, do not disturb the vessels subsidiary to the vasa vasorum—do not cut off the true vasa vasorum, nor strangulate them by your ligature. Tie the artery with a buckskin ligature very soft, and a little broader than the thickness of the skin; take care not to tie it too tight. Having done this, you may expect that the wound, in many cases, will heal by the first intention, where this fails there will be no suppuration about the vessel, and the wound will very soon close. You may expect a capsule to surround your ligature, if you have not disturbed the capillary vessels more than can be avoided. Or the vessel will be surrounded by an abundance of lymph, and the ligature dissolved.

In support of the above declarations we have to offer, in addition to the foregoing experiments, the following result, of our surgical practice. We have used the animal ligature for six or seven years; we have used it in many amputations of the limbs, and of the mamma. We have once tied the carotid—once the external iliac—twice the femoral—four times the radial—once the posterior tibial—seven times the spermatic; and, in no instance have we ever had secondary hemorrhage; never have we had a ligature to slip—we have never seen any thing of our ligatures after their application, and, often, indeed generally, our wounds have healed by the first intention.

Our experiments lead us to believe that we shall improve surgery, in cases of aneurism, by passing a seton of buckskin through the affected artery, instead of tying it. That in this way we may most safely obliterate a varicose vein. And, further, we should not hesitate to try the same method in aneurismal varix, from a wounded artery in blood-letting. It will be observed however, that we are in this paragraph offering our opinion without having tested it in the human body. We shall so far as possible ascertain its safety by fur-

ther experiments, upon living animals, as soon as time will permit.

In the application of the ligature we may have occasion for the following instruments.—For securing arteries, in amputations, the forceps and tenaculum have been spoken of, by many, as being equally useful. We have a decided preference for the tenaculum, and have to remark, that very often, there is not sufficient attention paid to the pointing of this instrument; a dull or clumsy tenaculum will greatly hinder the most dexterous surgeon, in taking up arteries of small size. We also prefer the small hooks, found in common pocket cases, to those, more straight, put into amputation cases. The former are more convenient, because a ligature looped about its shaft can be slipped most easily over the point—the latter is generally, in large arteries, passed too far through the vessel, and a difficulty occurs in slipping the noose or loop over its point. To avoid the unseemly appearance of the blood spirting over the surgeon and his assistants, in arteries that cannot be obstructed by the tourniquet, the surgeon should hold the fore-finger of the left hand over the mouth of the bleeding vessel, a little above the point of the tenaculum, which he holds in the right hand; and the assistant should learn to pass the loop of the ligature over, and below the point of the tenaculum, and direct it to its proper situation around the artery, which, in all cases, must be as close to the divided parts as possible; and he should do this without seeing either the mouth of the vessel, or the point of the hook, which is readily done by the sense of feeling, after a little practice. S. Cooper has given a drawing of a spring tenaculum designed to enable the surgeon to tie an artery without assistance. We believe this instrument will be found extremely limited in its use. Admitting that we can hold the vessel better with it than the common instrument, it will be found on trial, that, owing to its two curved points turning inwards, we could seldom get hold of the artery, should it be at all retracted.

Dr. Physick's aneurismal forceps is a valuable instrument, but in many instances, a curved needle, having an eye in its fore-part, like a sadler's needle, and such as was used by Cheselden, for applying ligatures to the enlarged tonsil, answers equally well. They must not be pointed, so as to endanger transfixing the vessels. Small silver probes with eyes, may be used, in most cases, of applying ligatures to uncut arteries in situ. But, after all, we are satisfied, from actual experience, that Physick's forceps armed, with a curved

needle is the best instrument. The tenaculum, the curved needle, and the forceps for holding the needle, are the only instruments wanted in taking up the arteries, excepting the common dissecting instruments, which should be in the best possible order, as the neatness and expedition of the operation will often depend much upon this circumstance. The pain will be lessened, and both the surgeon and patient will in these cases, gain much by the operation being done with reasonable expedition; we have always remarked that delay incommodes the surgeon, by suffering the cellular tissue, &c. to become injected with blood, and consequently more or less opake, by which the parts are rendered more obscure.

Of the advantages and disadvantages of the different kinds of ligatures.—We perfectly well remember to have seen ligatures applied, by practitioners of eminence, who had graduated at Edinburgh, about the middle of the last century. They were remarkable for their huge size; for large arteries, not very much unlike the twine seen around our writing quills. Yet, clumsy as they were, they possessed one advantage; they would not, in the first instance, cut the coats, and as in amputation the end of the artery was to be cut off, by ligatures of every kind which have been used, except the animal ligature, they answered very well, except so far as by their roughness and bulk, they irritated, and much increased the inflammation of the stump. But in cases of aneurism, these ligatures would not answer, unless they were cut away as soon as union was effected, and it is probable, in many cases, suppurative inflammation would have been excited by them; and we believe, actually did occur in the practice of Mr. Hunter, from such ligatures. Mr. Abernethy, in his work, recommends pretty large ligatures, because they will not cut the coats of the vessels; but, as we have already said, unless they were cut away, undue irritation and inflammation would follow, before they would cut their way out. Scarpa tells us, speaking of the application of ligatures to the carotids of two sheep, that "in one *he* used a flat ligature, and a small cylinder of linen, covered with cerate, between it and the vessel;" and upon examination, "the carotid was found covered, for the space of an inch and a half around the ligature, with a glutinous lymph," and "the internal coat of the artery immediately under the cylinder, far from being livid and contused, was sound, but thicker and softer than in the natural condition." Further, "the points of the surface of the membrane kept in immediate contact by the ligature, were united by vascular inosculation." Here is a clear proof,

and is supported by several other experiments, that the vessel was obliterated; and that the true vasa vasorum were neither cut, strangulated, nor inflamed, beyond the degree necessary for effusion of lymph, by which the artery was to be closed. In the other sheep, the coats were cut by a thread ligature, the vessel was inflamed, and a tendency to suppurative inflammation showed itself on the fourth day. Now, while these experiments serve to show the importance of preserving the parietes of arteries, they show with equal clearness, the advantage of a dissoluble ligature, which while it served the purpose of obliteration, was at the same time taken away by the natural operations of the absorbents. We trust we have already satisfactorily shown, that soft buckskin ligatures, applied moderately tight only, and not so small as to cut the coats, will wholly supersede the use of the complex measures necessarily attending the roll of linen, when applied to deep seated arteries, for the purpose of saving the coats from the ligature. And all ingenuity employed upon the construction of instruments for cutting ligatures, and removing them on the fourth day, in which Scarpa shows his usual acumen, becomes wholly useless, now that we have easily within our reach, a substance which will scarcely act as an extraneous body, when put around an artery; and after securing for us its office, is most safely cut away by the living absorbents, whereby we are enabled at once, with confidence, after tying an artery, to close the wound, as soon as we have applied our ligature, and give ourselves no further trouble about the result, so far as the ligature is concerned.

Of the fine silk ligature, and of the small flax ligature of Mr. Lawrance and Dr. Jones, we have already spoken, under the article pathology. We have only to say of these, that if our views of the animal ligature are correct, and we trust this matter will be decided, ere long, in favour of our opinion, nothing can be pleaded in support of the fine indissoluble ligatures. Whatever may be thought of the method of Œtius and Celsus, revived by Mr. Abernethy, of applying two ligatures, and dividing the artery between them, we trust we have shown, that as the animal ligature properly applied will not destroy the continuity of the vessel to which it is applied, no possible advantage can arise from dividing the vessel, and to detach enough of the artery to admit two ligatures, added to a division of the vessel by the knife, in the first place, must greatly increase the pain of the operation, and, in the second place, before the ligatures come away, both ends of

the artery must be cut of by attrition, or suppurative inflammation.

In cases of amputation, we believe that no ligature will bear a comparison with one of soft buckskin, of moderate thickness, cut from the skin about twice as wide as thick, and drawn between the nails of the thumb and forefinger, so as to make it more solid; but it should never be made round —this would cause it to cut the coats, make it less dissoluble, and more apt to slip off, all which we ascertained by actual experiment.

Of compression, as a mean of arresting hemorrhage in cases of surgical, or other wounds.—We have presented occasional remarks upon compression, in our foregoing observations, and therefore think proper to limit ourselves here to a very brief examination of this department of our subject.

An imposing list of successful cases of arteries obliterated by compression, might be collected from the best surgical authorities, and however useful this information may occasionally be, we must not lose sight of the opposite view of this subject, which would present its thousands, who have fallen victims to bad surgery, or sheer ignorance. We would admonish the young surgeon to look upon reports favourable to compression with mistrust, not that we doubt the veracity of reporters, but because sound experience shows the uncertainty of securing a wounded artery of any considerable size, by any other method than that of ligature; and for every case of success by compression, tens, and perhaps hundreds of unsuccessful cases have been lost sight of by practitioners.

One of the most important circumstances attending compression is, that of having, perhaps in all cases, to obstruct other vessels than the one wounded. It is true, we can in a considerable degree lessen the evil arising from this circumstance, by the graduated compress, laid immediately upon the artery, so as to raise the bandage passed around, from off a considerable portion of the limb, but not altogether. The obstruction of a considerable arterial trunk, must always considerably derange the economy of the parts beyond the obstruction; how obvious then, that the less we disturb the other vessels subservient to the same parts, the better. This applies as well to the veins as to the arteries, and perhaps in many cases, is of more importance, as it relates to the veins. We have elsewhere endeavoured to prove, that the arteries act with some increase of power, from meeting with reasonable resistance; but this is only to a limited extent, and the opposing force must not be applied too suddenly. The veins, on

the contrary, having but slender propelling powers, are easily overcome, and consequently if any considerable force is applied to a limb, the circulation is impeded, œdema and inflammation, even gangrene may follow. We are aware that much pains have been taken to contrive instruments which would serve to regulate the pressure, so as to make it against the vessel in particular, while but little is applied over other parts of the affected limb. These, it must be confessed, have, in some instances, done good; but their use is extremely limited, since, in a very great majority of cases, it would be better to relieve such patients by operation. There was a time when compression seemed more extended in its usefulness, because many entertained the opinion, that wounds of arteries might thus be healed without obliteration of their canal. But we have been led to believe, that no experienced surgeon will expect such a result; then, as we know the vessel is to be obliterated, how much better to tie the vessel at once.

One of the most important advantages to be obtained from compression is, its temporary use. By means of a tourniquet, or a simple strong string tied around and twisted tight, by means of a stick of wood, or the application of a finger immediately to the vessel, we readily for a time, control hemorrhage from a wounded vessel, and thus afford safety, in cases of wounded arteries, till we have time to adopt more efficient measures. Mr. C. Bell, Oper. Surg. (406) says, that compression is generally sufficient in gun-shot wounds; that arteries contused are less disposed to bleed, we readily admit, but are of the opinion that Mr. Bell's assertion is too positive; and he who would rely upon it, will occcasionally meet with disappointment. We have already noticed two cases which ended fatally, from secondary hemorrhage, in gun-shot wounds; one from Mr. A. Cooper, and one which came under our own notice. Boyer tells us, in his surgery, vol. I. (132) that he has frequently seen compression, from being too great, prove fatal; and that, in some cases, "the strongest compression is hardly sufficient to arrest a small hemorrhage;" "to continue the compression, in such cases, will sometimes lead to tumefaction, convulsions, &c., which may prove fatal." This author also informs us that when we have closed a vessel, by compression, "we must continue it for a long time, otherwise the patient is exposed to a false consecutive aneurism; and, he tells us, he saw bleeding take place upon removing compression from the brachial artery, after the vessel was supposed to have been healed. This information is valuable, and should have its influence on our conduct. Different authori-

ties give a case, from J. L. Petit, in which a coagulum was found in an artery two months after it had been wounded. We have some account of this case in Boyer's Surgery, vol. I. (127.) This author says that there was presented, by "J. L. Petit, to the Royal Academy of Sciences, the brachial artery of a man who died suddenly two months after he was cured of a wound of that vessel. The lips of the artery had not united to each other, the blood was stopped by a clot, which closed the vessel." We are also told, where wounded arteries have been treated by compression, and the vessel is supposed to be closed, "the surgeon feels perfectly secure, but in three or four months, sometimes later, the clot, by which nature has arrested the hemorrhage, becomes detached, and the blood is injected into the cellular texture, forming an aneurismal tumour." No one, we believe, will hesitate to think with us, that we can never so effectually keep in contact the inner surface of arteries, by external pressure, as by ligature around them; when we add to this the fact, that compression is in itself a source of irritation, with how much caution should we listen to the favourable accounts occasionally met with, of cures by compression. Notwithstanding the vast opportunity which was before Boyer, and indeed the vast amount of information given by him upon surgery, we find him clogging some of his pages with advice which detract from his work. At page 132, vol. I. we are told, "that for some hemorrhages, lateral pressure is the only remedy, for example the intercostal artery, and the internal pudic:" what would we think of a surgeon who would not know, from his anatomical knowledge, where to cut for securing the intercostal, and who has not heard of Dr. Physick's success in taking up the pudic.

Where compression is allowable in wounds of smaller arteries, and in parts where a tissue of vessels bleed from debility of the parts at the time; or from their natural want of support, as in the parts about the rectum, axilla &c., the sponge is a valuable article. J. Bell says, of more value than the needle. Mr. A. Burns, (342,) relates an interesting case of deep seated aneurismal vessels about the jugum of the face, in which his brother could not entirely remove the disease by the knife, and a profuse bleeding continuing, was arrested by binding down firmly a piece of sponge. C. Bell, (391,) advises sponge to be removed from wounds, so soon as suppuration commences. We have often found this impracticable. Our experience with this article convinces us, that, next to soft leather, sponge is the least irritating substance

which can be applied to a raw surface; and we have again and again observed in healthy subjects, organized lymph shooting into its interstices, or some of the granules or fibres drawn by attraction into the interstices of the sponge, and forming so close a union with the parts that it could not be removed, without great violence, until there was free suppuration established in the wound. This adhesion by lymph, and cohesion of the sponge to the flesh, takes place in a few hours, and is an objection to the employment of sponge where we wish to remove it speedily. We advise, therefore, in cases requiring temporary pressure from some soft body, to cut surfaces, which we wish to heal without any, or as little suppuration as possible, to use lint in preference to sponge. The lymph will soon loosen the lint, but it will closely confine the sponge to the divided parts. Where the hemorrhage is obstinate, and the patient at a distance, the sponge, having the greatest restraining powers, should be preferred. We have mentioned a case, in which, a sponge forced into the leg, led to tetanus and death. C. Bell, has seen caries produced by sponge. Sir A. Cooper, in his lectures, (90,) mentions two cases, in which mortification seemed to have been produced by the use of sponge. In short, sponge has its value, but let us avoid it wherever the ligature can be used. In many cases we believe bleeding surfaces ought, in preference to the sponge, to be secured by animal ligatures, passed through the wound backwards and forwards, in a manner somewhat similar to that recommended by C. Bell, vol. 2, (388,) in cases of callous ulcers, which bleed dangerously. It is to be done, by passing in a crooked needle armed with an animal ligature. The needle must be passed through the part, a little below the surface, at distances of about three quarters of an inch, and the ligatures thus placed parallel to each other, are run first from one side to the other, that is, having passed it through from one side, we carry it back again to the other; in this way, we shall include the parts in loops of the ligature, and can tie them securely.

We must not here lose sight of the fact that, in cases of wounds of arteries in a state of ossification, we must rely on compression; and we think that in large arteries, the best material is a little roll of soft leather, such as we use for ligatures; next we would use compressed sponge, and lastly, lint passed into the vessel. We are reminded in this place of the division of Boyer, of pressure upon arteries, into two kinds, the *lateral* and *direct*. A moment's reflection will

convince us, that the greatest advantage obtainable from pressure must be *direct*. For instance, if we amputate a thigh, and find an ossification of the femoral artery, after plugging it, we can apply a direct force to the stump, which will act against the end of the artery : but suppose the same patient, had been wounded in the same artery, we could not apply compression with equal advantage; we could not by any of the means usually employed, apply an opposing force to the blood, which would act in the directly opposite direction—here then is obviously a case of great danger, yet, we would not despair in such a case. We believe that a case can scarcely be found, in which we could not by due caution, approximate the sides of the vessel by means of a large soft buckskin ligature, drawn slowly, and not too tight. And if no better alternative presented, we would slightly detach the end of the artery, and cutting two half-round small pieces of thick untanned hide, lay one piece below, and the other above the artery—the two flat sides being opposed to each other, and the vessel between them, it will follow, that when we tie a ligature around, we shall flatten the end of the artery between the pieces of hide. The artery being cylindrical and brittle, may have its sides more readily and effectually brought into contact, thus flattened ; but drawing in the sides of a brittle cylindrical body, must, almost necessarily, break it more or less. Having in this way, secured the artery the wound may be closed, and the hide and ligature will be removed, in due time, by the absorbents. We need perhaps hardly remind any of our readers, of the total incompatibility between tannin and gluten, which would render any skin, subjected to the action of tannin, totally unfit for the purpose of ligatures.

Of the influence of retraction and contraction of arteries, as a means of obviating hemorrhage.—Our daily experience convinces us, that arteries possess powers of retraction and contraction, which greatly facilitate our endeavours to arrest their bleeding. The risk of hemorrhages however will not, in all cases, be proportioned to the amount of retraction and contraction, since we know that if the artery be much injured by contusion, or by gangrenous inflammation, and the vessel not too large, blood will not be given out, although the mouth of the vessel remains open. In other cases, if the artery be divided by a sharp edged instrument, there will sometimes be but little contraction or retraction, and small muscular arteries may slowly diffuse blood into surrounding parts, till dangerous diffused aneurism without pulsation is

formed, of which an interesting account has been given by John Bell, Abridg. Surg. (85, &c.) Moreover, the very circumstance of a power of retraction in arteries which, in general, renders it a favourable circumstance, may occasionally lead to danger. Is an artery wounded which we may reasonably expect, from its size, to close by retraction and contraction aided by compression, and should the vessel be drawn considerably within a muscle, or cellular mass, where it cannot be seen, where even a sponge forced into the wound will not touch the mouth of the vessel; and it so happens that no lacerated portions of cellular texture are situated around the end of the artery, we shall have a combination of circumstances which will lead to danger. Under such circumstances, we may have a state of things more difficult to manage than where the artery is much larger. If there be a punctured wound we should dilate it reasonably, if possible, to the bottom. If the wound be already sufficiently large, we should open it freely, and by the finger expose the wounded surfaces to the air, even for an hour or two if necessary; the reason for doing which has been already explained. And if the source of bleeding can be clearly marked, we should endeavour to get at the vessel, and by merely pricking it with the tenaculum, we shall often succeed in making it contract or cease to give out blood. These cases are fortunately very rare, and the sponge will mostly succeed; but must not be blindly persisted in, if it does not in reasonable time answer our purpose. We have been well aware of the fact, since the time of J. Hunter, that the smaller arteries near the heart are more under its influence than those more remote; and consequently we have less to expect from the powers of contraction and retraction, in the former; and hence it is in part that, "the external thoracic or pectoral artery," will sometimes, according to J. Bell, (86,) fill the "axilla completely with blood." There are however some exceptions to the circumstances just pointed out, and which have been noticed in a former part of this essay, (to wit,) the female mammæ, and the genital organs, in both sexes being endowed with an *erectile action*, are less under the control of contraction and retraction when wounded, and this has an important bearing upon our practice.

Of the potential cautery, as one of the means for arresting hemorrhage.—We need scarcely remind our readers, of the abuse of this remedy by the ancients—more modern times afford cases of appalling cruelty, in the use of it. And more extended experience, while it condemns the use which was

once made of this remedy, serves to point out its utility, in certain cases. And we do not hesitate to say, that of late much more suffering has been imposed upon some patients by styptics, than would have attended the actual cautery. Remedies of this description are, not unfrequently, used by persons unskilled in surgery, where there is no occasion for their use; and as they do not carry with them the terror of hot irons, patients often manfully bear the most smarting articles, as unnecessary as they are painful.

Dr. Smith, in a note, Bells' Surgery abridg., informs us, that Sir A. Cooper used with success, the actual cautery in a dangerous case of bleeding from the foramen incisivum. Mr. Hay of Leeds, was in the habit of using it in tonsilitic bleedings; C. Bell (387), advises the use of the cautery in cases of spongy tumours, and bones bleeding obstinately. In the New York Journal, No. 18, is a case in which it was used, to the meningeal artery, and we have lately read, in a periodical work another case. Both these cases terminated fatally—dissection in both seemed to forbid the idea of the cautery having been hurtful. We have no disposition to call this in question, in these particular instances, but would offer our protest against a practice which, to say the best of it, may for aught we know, do serious injury to the brain, and one for which there is no occasion whatever; there is nothing new in these cases; they have often occurred. In most cases of wound of this artery attended with fracture, we can safely separate the dura mater a little, and pass in a small hook and draw out the retracted artery and tie it—and indeed, in most cases merely passing the hook through the vessel will check its bleeding as effectually as the cautery. If the vessel has been contused, and there is not much separation of the dura mater, a little lint will repress the bleeding; and indeed, has often succeeded. In obstinate cases, we would even prefer setting on a very small trephine, such as we use on the metatarsal bones, and cut away a portion of the bone. Lastly, if there be rupture of this vessel without fracture, we should, in the application of the trephine, set it immediately over the vessel, if possible, and take care to cut through the bone, first at the upper side of the perforation, and we shall thus have the artery exposed over the extent of the perforation; and if wounded by the trephine, it will be in the most favourable part, and enable us to apply a ligature. Boyer, in his surgery, (130,) mentions the case of a patient bleeding dangerously from the ranular artery, who was

instantly relieved by a surgeon introducing, unknown to the patient, a heated iron into the mouth.

We are reminded by the case of Mr. Cooper, just noticed, of two cases of hare lip, in our own practice. In both these cases, a portion of the palatine bones was wanting; and part of the superior maxillary under the nose was attached to the nose, and to this was attached a considerable portion of the lip, forming a most unseemly snout. Upon cutting this knob off, preparatory to approximating the sides of the divided lip, the knob was cut off with a strong scalpel; an artery of very considerable size bled furiously. It was found necessary only to rub the surface a little roughly, and press upon the part with the finger a few minutes; and then bringing the sides of the lips against it, no trouble was experienced from the vessel. We would advise in all such cases to prepare the lip first for sewing, and then take off the knobular projection.

Of astringents as one of the means for arresting hemorrhage.—We have taken occasion, in a former part of this essay, to say something respecting the use of astringents. They have their value in wounds in relaxed parts, but are mostly attended with much pain, at least some of them; and are not unfrequently attended with unpleasant irritation and inflammation. In languid habits this may, occasionally, be useful, by exciting into more vigorous action parts that are sluggish. Strong alum water, warm wine—brandy, or brandy and water, Turlington's balsam, spirit wine, and similar articles are frequently used, for restraining an oozing from ulcerated surfaces, or wounds of relaxed parts. They doubtless are sometimes useful, as we may infer from their utility in cases of epistaxis and uterine hemorrhage, but we should not forget that, in irritable habits they may do much mischief, by over stimulation; and are never to be trusted where arteries of any considerable magnitude are wounded. Mr. J. Bell, Abridgment of Surgery, (45,) speaks favourably of agaric, on account of its absorbing qualities, we have no hesitation in saying, it has no advantage over sponge or lint. Boyer, (128,) Surgery, speaks of sp. wine, and sulph. copper, as articles which may, occasionally, be usefully applied in moderate hemorrhage, but which sometimes produce considerable irritation.

Of Caustic as one of the means for arresting hemorrhage.—The application of caustic, for restraining hemorrhage, is very much limited in its use. But we have occasionally, found it extremely useful, particularly the nitrate of silver,

for restraining bleeding from fungous tumours, even where lint, and other astringent applications were ineffectual. We have also succeeded with it in bleeding from the socket of a tooth, by pressing it down and filling the cavity with lint. We are reminded here of a fact, related in Mr. Shaw's Manual for Students, (308.) The artery within the lower jaw had been torn in the extraction of a tooth; two respectable surgeons were talking of tying the carotid; a student present slipt the tooth into the socket, and thus at once put a stop to the hemorrhage, and the consultation. Frightful tales might be told, from good authorities, of the accidents and disasters which have resulted from surgeons relying on caustic, of various kinds, for the suppression of hemorrhage; but we will not conclude this division of our subject by the recital of the unskilful deeds of the profession in former days; nor with the gloomy picture of the sufferings of their patients. Let us rather console ourselves that these misdeeds on the one side, and the misfortunes of the other, have given place to a more enlightened philosophy, and a more judicious practice formed on experience.

Of the collateral circumstances which favour the healing of wounded arteries.

Of medical treatment.—It will readily occur to the well informed reader, that to elucidate this subject, in all its bearings, would require nothing less than a full investigation of the subjects of irritation, inflammation, and fever. To enter into the subject in this extended sense is altogether incompatible with the limits of this paper, we shall, therefore, refer the reader to works upon those several subjects, and content ourselves with a brief exposition of some circumstances which have occurred under our own observation.

We believe that there has been much mal-practice by surgeons in wounds, as well those attended with wounded arteries, as in their operations, in using depletory measures before re-action came on. It will be found by the careful observer that, in many cases of healthy patients wounds, will heal as well, often better, if nearly the whole is left to nature. We might turn to the works of Mr. Abernethy, and many other, of the most respectable authorities, for instances so frequent as to amount to a rule of practice, that, where, owing to a wound or surgical operation, inflammation or fever might be expected as a consequence, free depletion by the lancet and otherwise, was used with a view of preventing the occurrence of fever, or inflammation, or at least of

controlling them within safe bounds. Now, we think our experience warrants us in asserting that, in very many cases, this will lead to the very evil we wish to avoid: and in a great majority of cases of operations or painful wounds, if we energetically remove pain and irritation, if they present themselves, by full anodynes, no undue excitement will take place, either in the part injured, or in the general system. Whereas, by prostrating our patients unduly, we render the system more excitable, and increase the tendency to inflammation, and febrile excitement. Let us give a case in point, a patient of debilitated habit, from a sedentary life, was operated on for cataract; a few hours afterwards he was bled to sixteen ounces, and rest, darkness, and strict diet enjoined, cathartics were also ordered. On the fourth day the most violent pain and inflammation came on in the eye, with general fever. The practitioner, thinking he had done enough to prevent mischief from this state of things, contented himself with aperients, leeches, &c., with warm water to the eye. The patient continued to suffer violent fever and excruciating pain for four weeks, when we were called in, and found it necessary to use copious depletion by the lancet, calomel, antimony, digitalis, &c. The patient improved every hour under this change of treatment. We have so frequently seen cases similar to the one just related, though seldom as violent, that we feel a degree of assurance in saying, the premature bleeding, &c. was probably the cause of the fever which succeeded.

But if we are disposed to leave our surgical patients pretty much to nature, as to the general treatment, until there are constitutional symptoms to justify our interference, we are bold to advise, and practise, the most prompt and decisive measures when symptoms arise to require it. We do not wait till the system is prostrated by fever, or until a painful wound has run into dangerous inflammation, on the contrary we meet those symptoms, so to speak, at the threshold; and thus, in most instances, keep them within moderate bounds. It is not easy to conceive how men of opportunity, with the best talents for observation, should so long have followed in the foot-steps of each other, in employing means for the removal of that which did not exist, that is, attempt to cure fever or inflammation not present. But when we take into consideration the well known fact that, by reducing the system, which is not labouring under fever, below its natural standard, we increase the liability to disease, since we destroy the balance of the natural powers, how shall we account

for that infatuation which would lead us to make a patient more healthy than he is in his common state of health. In a word, in general treat wounds, whether surgical or accidental, according to the state of the system which attends them, neither attempting to prescribe in anticipation, nor depend upon any other than measures proportioned, according to our judgment, in each case, to the violence of the symptoms present.

We wish, in concluding, to point out one exception to our general rule, we are well persuaded from the success attending the practice of Valsalva and Pelletan, in cases of aneurisms of large arteries, that our patients should be reduced, as far as circumstances will admit, in cases wherein we tie the arteria innominata, and perhaps, the subclavian. The force of the blood in arteries so large and so near the heart, must be a source of danger from the liability which it must produce to secondary hemorrhage. We would in these cases, enjoin the greatest possible stillness, the most reduced diet, abstract blood according to circumstances, &c. taking care to keep the circulation much reduced. And, indeed, in all operations, upon large arteries, we must carefully bear in mind the important advice of Dr. Jones, in regard to keeping the patient still, not because we have any fears of our ligatures being thrown off, by the increased action of arteries, in a state of fever, but because fever will generally more or less interrupt the process of healing by the first intention, and exertion will lead to fever. We should, in all cases, pay particular attention to the bowels. The French are partial to vomits which, in cases of wounds of a serious nature, we suppose tend to remove a sluggishness of the primæ viæ so apt to attend such cases.

Of Regimen as one of the collateral means which may aid in the healing of wounded arteries.—That regimen has a powerful controlling influence in the prevention of fever and inflammation, has been long and universally acknowledged. But more recent experience fully supports us in saying, in healthy subjects no advantage will arise from a reduced regimen, with a view to prepare the system for an operation. Neither will a patient be less liable to inflammation and fever, in case of accidental wound, on account of his having been on a reduced regimen. We might judge, a priori, that no state of the system could be more favourable than that of perfect health, and to admit the existence of this perfect health is pre-supposing the patient to have lived regularly, otherwise his health cannot be considered perfect. Our practice in the inoculation for small pox, in former years, most

clearly supports our opinion, since it was well known after trial of both methods, that no advantage arose from dieting such patients before the period of their innoculation. From the period of inoculation it was indispensably necessary to diet strictly.

It may reasonably be expected, and our experience comes to our support, that where a person undergoes a severe operation, or meets with a serious accident, there will be some degree of feverishness; and hence the advantage in all cases of full, or good habits, of confining them to a regimen strictly anti-phlogistic. In addition to this view of the subject, the disturbance which takes place upon the nervous system, and often upon the sensorium, is such as to disable the stomach from performing its function well, and hence another reason why we should withhold food except such as is easy of digestion, and affording but little nutriment. To avoid trouble with ungovernable persons and children, we have often benefited our patients by reduced doses of antimony, with a view of blunting the appetite. We beg to be understood here as speaking upon general principles; in some particular habits, disposed to low action, we must, from the commencement support our patients with cordial drinks, but light and nourishing food. And in cases of debilitated habits, as has been pointed out by Dr. Good, opiates and cordials by imparting vigour to the system arrest hemorrhage. These cases are rare, and we will seldom injure our patients by too low a regimen.

We have sometimes confined our patients to toasted bread and water for several days—wholly forbid animal soups, and all articles of solid diet, wines, or spirituous drinks, of every kind. We believe that a state of the system tending in some degree to a state of inanition has a most salutary influence upon wounds. We leave to others to account for the rationale, by which abstinence is seen to exert a decided conservative influence in the animal economy. In concluding this paper, extended beyond our expectations, we must content ourselves here by insisting on the facts, whatever may be thought of our theories.

Art. II. TRANSACTIONS OF THE MEDICAL SOCIETY OF VIRGINIA.

The five following papers by Drs. Watson, M'Caw, Archer, Dove, and Briggs, were read before the Medical Society of Virginia.

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No. 6. *Memoir on Sarcocele, with a case.* By G. Watson, M. D.

Amongst the diseases of the organs of generation, which it has fallen to my lot to witness, the most formidable, without question, is sarcocele. I allude to that species which often acquires an enormous size, is confined chiefly to tropical regions, and has been found, with but few exceptions, connected with elephantiasis. The mind is impressed with a horror, scarcely to be described, at a monstrous change in the aspect of parts, about the condition of which nature seems to have endowed us with an extraordinary degree of sensibility. And the sufferer from this malady shows, by his conduct, that time and necessity have but little effect in reconciling him to its consequences. For in the case which I have seen, at least, he was willing to abandon the haunts of men, and all the aids and comforts they can afford the sick, rather than encounter the feelings, in others, which he saw too plainly, his presence never failed to excite. And for months before his death, he took up his abode of choice in a stable, from which he would crawl, at his own time, in obedience to the necessities of nature.

Larey, the celebrated French army-surgeon, who has paid much attention to this disease, knows of but one case which had its origin in Europe. This was in the person of Mr. Charles De Lacroix, formerly a minister of foreign relations in France; all the instances within his knowledge, with this exception, having come from Asia and Africa. In further proof of the rare occurrence of this disease in Europe, we find no mention of it among the diseases of the testicles, in the works of those systematic authors, who have written the most at large on that subject. I know of no case of the disease having been met with in the United States, except the one I am about to describe.

Of the remote cases of sarcocele, little more can be said, than that it is a disease of the climate, which we fairly infer from its being confined to hot countries, and that it is inti-

mately connected with that state of the system which produces elephantiasis. Its connexion with this last disease is so close that, Larey says, amongst the great number of individuals which he met with in Egypt, labouring under sarcocele, there was not one who did not at the same time show the symptoms of elephantiasis. In the sixth volume of the Medico-chirurgical Transactions, a very remarkable case is given by Dr. J. M. Titly, of the island of St. Christopher, with some remarks on the disease generally; and he speaks of it as one of the *symptoms* of elephantiasis.

However it may be brought about, the fact appears to be, that from some unknown cause or other, a morbid action takes place in the vessels of the scrotum, which leads, by those subtle processes of the animal economy, which are most likely, destined forever to elude the scrunizing eye of the pathologist, to an enormous growth of that part. In the case described by Dr. Titly, the tumour had reached the weight of seventy pounds. And Larey says, he saw many in Egypt, the smallest of which weighed fifty pounds. Ten or twelve amongst them were supposed to weigh one hundred pounds, or more.

Sarcocele of this description is not confined to the male sex. For the labia externa of women, having a strong resemblance in structure to the scrotum, are subject to the same disease. Larey describes a case which he saw in Egypt, and gives a picture of it, which leaves no doubt of its identity with sarcocele.

The only remedy in this disease, in favour of which any claim to success has been made, is removal by an operation. And this, if we may confide in the accounts which have been given, has generally proved successful.

I wish to draw the attention of the society to a case of sarcocele, which was the frequent subject of my notice, for about twelve years. The patient was a negro man, born in the county of Powhatan, Virginia. He had spent the whole of his life in Richmond and its vicinity. He did not know his age exactly; but when attacked by the disease, he must have been, from the best calculation which could be made, under forty years old. During the previous part of his life, his general health had been uncommonly good. And from the commencement of the growth of the sarcocele, to the end of his life, he remained perfectly free from all complaints, except such as were fairly traced to a local disease.

Some years previous to the beginning of the sarcocele he had been severely afflicted with gonorrhœa, having suffered

distressing attacks of it at several different periods. Obstructions of the urethra, and their usual consequences followed. He was advised to use bougies; but this practice, not being skilfully conducted, he derived no benefit from it. When I first saw him, in the winter of 1810, the prepuce, and the integuments of the penis were indurated and thickened sensibly; the scrotum was considerably enlarged, and formed a tumour equal in size to the two fists. These parts were entirely free from tenderness, or heat, or any of the marks of inflammation. The spermatic cords were distinctly traced from the upper part of the tumour into the abdominal rings; the tumour was equal in size and appearance on both sides, and the sensation imparted to the touch, and the total absence, at all times, of the disturbances of the alimentary canal, which mark the descent of the viscera into the scrotum, satisfied me that there was neither hydrocele nor hernia in this case. Some scars on the front and lower part of the scrotum, a fistulous hardness in and around these scars, and an actual stillicidium of urine through a very small opening in one of the scars, led to the idea of urinary fistula; and upon sounding the urethra, an obstruction was met with, about two inches from the orifice, which proved to be perfectly impervious to any instrument which could be tried. This discovery was considered a satisfactory explanation of the symptoms. The obvious practice was to remove the obstruction in the first place. But there were circumstances in the case, not of a medical nature, which prevented any regular method of treatment, with this view, from being put in practice.

The disease continued to advance with a constant and increased rapidity. In a few years all distinction of the external parts of the organs of generation was obliterated, and the scrotum, testis, and penis, were involved in one tumour. The opening of the prepuce formed what has been very aptly called, a navel-like appearance on the fore and upper part of the scrotum. On the 25th of May, 1822, it had acquired the following dimensions. From the pubes to the verge of the anus over the tumour, was forty-eight inches. A line passing over its greatest diameter, and cutting the above at right angles, was forty-two inches. From the symphysis pubis to the opening of the prepuce, was eight inches; from thence to the most dependent part of the scrotum, was fifteen inches. The shape of this enormous mass approached the sphere more nearly than any other regular form. It was harder, and more resisting to the touch than the sound and

fleshy parts of the body, and the hardness was nearly the same in every part. Pressure with the fingers, continued for some time, left the pits which characterize œdema. The spermatic cords were distinctly traced from the abdominal rings into the body of the tumour. But upon the most careful examination, the testicles could not be distinguished by the touch, nor was it possible, by any degree of pressure, to excite in the patient, the feeling peculiar to that gland. An artery was distinctly felt on each side, entering the tumour from the perinæum, and immediately beneath the skin. It appeared to be about the size of the ulnar artery at the wrist. About midway between the opening of the prepuce and the bottom of the scrotum, there was an ulcer nearly circular in its outlines, penetrating an inch and a half into the substance of the tumour, red, healthy looking, and discharging pus from its surface. Its diameter was between five and six inches. Its extent had been within a short time greatly increased by an attack of sloughing. Upon the surface of this ulcer, at its upper margin, and close to each other, were three preternatural openings, giving vent to nearly the whole of the urine. This ulcer seems to have been produced by irritation, excited from the constant efforts of nature to close the fistulous openings by the healing process, and the consequent impediment to the escape of the urine. For several years past, he had been subject to suppression of this discharge, which either yielded spontaneously, or to a palliative treatment. But of late, the preternatural openings had become so large and well established, as to secure him against this evil. From the patient's own statement, a small quantity of urine still passed by the urethra. When he was standing erect, the tumour reached nearly to his knees. It could not occupy the space between his thighs when they were separated to the utmost extent; but rested upon them either before or behind. When walking, which he could do with difficulty to the day of its removal, he threw it behind, and supported it on the back part of his thighs.

On the 26th of May, 1822, at one o'clock P. M., I proceeded with the concurrent advice and assistance of several medical friends, to the removal of the sarcocele above described. The poor patient, though he expressed an unshaken desire to have the operation done, was greatly overcome by fear. Some time was spent in encouraging him, and waiting for him to summon to his aid the resolution and fortitude which the occasion required.

He was at length placed on the table, on his back, with his

legs separated to the greatest extent, and even then there appeared to be a want of room, between the upper and inner part of the thigh and the tumour, to make a deep and extensive wound, which was certain to be followed by a considerable flow of blood. I placed myself in the erect posture, between the patient's legs, as the most advantageous position. It had been previously determined to avoid wounding the penis, and to save as much integument as possible. With this view, the incision was commenced near the verge of the anus, and extended forward to the opening of the prepuce. This cut passed across the spermatic cord, and below the penis. The cord was easily felt, and with a large needle, a ligature was passed around it, including all its parts. The cord was then divided below. The neck of the tumour being half divided on the right side, exactly the same steps were taken on the left, and the sarcocele was removed. In the progress of the operation, the arteries were secured as they were divided, by the needle and ligature. Though carefully and deliberately sought for, the presence of these vessels could not be discovered by the touch; it was only announced by the flow of blood. The use of the needle was preferred as affording the most prompt and certain means of restraining the hemorrhage, and on the right side, on which the operation commenced, was resorted to many times. On the left, very few vessels required the ligature. I cannot account for this circumstance. The soft and yielding surface of the wound admitting only an imperfect pressure to be made on the mouths of the arteries until the ligatures could be applied, the patient, in spite of all the despatch which could be used in getting through the operation, which lasted from fifteen to eighteen minutes, was thought to have lost between two and three pounds of blood. But in this respect, accuracy could not be approached very nearly. During the operation, syncope took place several times, making it necessary to resort to stimulants.

The ligatures on the spermatic cords, when applied, were intended to be temporary. But as the patient was considerably exhausted when the operation was over, and did not complain, even when interrogated, of any pain from the pressure, it was resolved to let them remain for the time, and get him to bed. And no symptom occurring afterwards, which called for their removal, they remained until the termination of the case.

The wound was first dressed with a mild plaster, a com-

press, and bandage; and afterwards, to promote suppuration, with a soft warm poultice.

The retraction of the skin about the wound was very considerable, from its having been kept greatly on the stretch, by the size and weight of the tumour. The wound covered the whole of the perineum, the part naturally occupied by the scrotum, and the groins.

After being put to bed, the patient continued cold on the surface and extremities, with a feeble pulse. He was drowsy, but easily roused from sleep, and perfectly correct in the operations of his mind. He had neither thirst nor appetite, but upon being urged to do so, he took both food and drink, in very small quantities at a time. Vomiting was easily excited on these occasions, showing great weakness and irritability of stomach. He continued in this state, with little variation of symptoms, until Wednesday, 29th, when upon seeing him in the morning, a material alteration for the worse was observed. He lay in a state of stupor, from which he could not be roused. On the evening before, some slight appearances of gangrene had been observed in the cellular substance in the wound. It had now become more manifest to the eye, and the smell was perfectly characteristic of mortification.

From the time of the operation to the present, the patient had complained of no unusual distress from the wound, and had continued remarkably composed in body and mind. At half past four o'clock, P. M. on Wednesday, 29th, he expired, having survived the operation rather more than seventy-two hours.

The pain inflicted by this operation, the loss of blood, and, perhaps, the sudden removal of what had constituted so large a proportion of the body, made so strong an impression on a system previously reduced to a state of great debility, by long continued disease, that it could not be resisted. But there is, in my opinion, every reason to believe, that this operation would have terminated in a very different manner, if the patient had submitted to it some months sooner; which he had often been strongly urged to do, when there was a good appetite, a good digestion, and a considerable stock of general strength remaining. All these favourable circumstances had disappeared in a great degree, for some months before the operation; and he only consented to have it done, when convinced in his own mind, that he must soon die without it.

The sarcocele, when removed, weighed forty-seven pounds. It has been mentioned, in the description of the tumour, that

the urine passed by fistulous openings, terminating on the surface of the large ulcer in front. In the operation, whilst the dissection was going on, immediately below the opening of the prepuce, and between it and the ulcer, something was divided, like a large artery in the dead body. The open mouth of a tube presented itself, which would have admitted the end of the little finger. This was soon ascertained to be the largest of the fistulous openings, leading from the urethra.

In the dissection which was made after the operation a director being introduced into this opening on the tumour, passed directly towards the ulcer. The passage was slit up, and found to diminish very rapidly in size. Within an inch and a half of its termination in the ulcer, it divided, and ended in two of the openings mentioned in the description of the tumour. Near this, and in front, referring to the position of the tumour previous to its removal, was found another fistulous opening. This was much smaller than the other, insomuch that it was not observed during the operation. This being traced and slit up in the same way, was found to terminate in the third opening in the ulcer. The inner surface of these fistulous canals had become a perfect mucous membrane; and resembled in a striking degree, the villous coat of the stomach.

The testes, were completely imbedded in the upper part of the tumour. They were readily found by tracing the spermatic cords, the ends of which were easily recognised on the surface of the wound on the tumour. They had undergone no change decidedly morbid. If there was any deviation from the natural appearance, they were rather smaller, and more flaccid than they ought to have been. A drachm or two of serum was found in the tunica vaginalis testis of one side.

The sarcocele being extensively laid open for the purpose of examining its structure and appearance, presented a mass of cellular substance varying somewhat in its density in different parts, and loaded, not with fatty, but serous matter. This was so much the case that several gallons of liquid flowed from the mass during twelve or eighteen hours that it remained on the table after the dissection was finished.

After the death of the patient the parts concerned in the disease were likewise examined. And although from the extreme heat of the weather, and the intolerable smell produced by the gangrenous state of the cellular substance, which has been previously noticed, it was impossible to conduct the dissection with the care and deliberation which was desirable,

the following facts however were clearly and distinctly ascertained. About the middle of that portion of the urethra which is embraced by the scrotum, there was an obstruction which could not be passed by a small probe; and immediately behind this was situated the commencement of the largest of the fistulous openings, above described. About an inch in front of this obstruction was another; and behind this the smaller fistula had its origin. From this last obstruction to the end of the penis the urethra was so far obliterated that it could not be tracked with the probe. Yet, as a small quantity of the urine passed that way the obliteration could not have been complete. That portion of the urethra between the hindmost stricture and the bladder was very far from being in a natural state. It was much contracted in its diameter and rough and uneven on its surface. The grooved director passed along it with difficulty, but could not be made to enter the bladder.

From the above history it appears that the following train of causes and consequences operated in the case. Several severe, and probably badly managed, attacks of gonorrhœa terminated in obstructions of the urethra. No effectual means of relief being put in practice, suppressions of urine and fistulæ in scroto were the consequences. From the constant irritation kept up by the diseased state of the urethra and the passage of the urine through the fistulous openings a morbid action was set up in the scrotum, ending in the production of the enormous mass of disease above described.

I am far from ascribing to diseases an importance merely on account of their singularity: for in truth the rare occurrence of a disease takes from its general importance, unless it may establish some fact or elucidate some principle which may give it value. It has not therefore been with a view to excite astonishment that I have recorded this case, and now turn the attention of the society to it. But I consider its value if it has any, to consist in the following considerations.

Naturalists say, that in exploring a new region, it is as interesting to know, that it contains the same objects which exist in the old, if that should be the case, as to discover the existence of things altogether new. The remark is equally true when applied to medicine. The naked fact of the existence of Sarcocele in Va., is therefore of some importance in its medical history. But in another point of view I regard it as of much more consequence. If, as is confidently believed, the sarcocele was produced by fistula

in scroto, or stricture of the urethra, or both together, it adds one more example of the extraordinary, and unexpected consequences of the diseases of the urethra. Hence we are led to the important practical conclusion, that whenever, we meet with singular, and anomalous diseases about the organs of generation we should not fail to examine the urethra, before we make up a final opinion, or institute our plan of treatment. And in like manner when we encounter complaints or symptoms which we cannot understand or succeed in removing, in patients who are known to have stricture, we should hold in mind the possibility of a connexion between them, and regulate our conduct accordingly. For if obstructions of the urethra can excite in a neighbouring part, a morbid action which terminates in the production of a tumour equal in size and weight to one third of the whole body we could not be surprised at its producing in the remote parts of the system, in a less degree the most decided effects.

No. 7. *A Case of Necrosis of the Tibia.*

By W. R. M'Caw, M. D.

Being persuaded that a detail of interesting cases, throws more light on medical science than the most ingenious and fanciful theories, I have thought proper to select as the subject of an essay, a very uncommon case of necrosis, that occurred in 1822.

Simon, a boy aged 12, of a scrophulous habit, (the property of Mr. ——, of this city) playing with one of his companions, received a kick from him in sport on the tibia, which at the time did not occasion much uneasiness. Two or three days afterwards, as I was informed, he began to complain very much of the injured part, and the integuments commenced swelling, and proper measures not having been taken to arrest the inflammation, suppuration came on, and when I was called in on the 6th of March, three or four weeks after the receipt of the injury, I found that the matter (not having a proper vent) had travelled along the tibia, and had discharged itself by three openings, each an inch apart. On examination, I discovered that a probe might be readily passed from the upper to the lower opening. The parts adjacent were quite hollow, and from the sinus a copious discharge proceeded.

The boy was in a very emaciated condition—his tongue a

good deal furred, skin hot, pulse small, and one hundred and twenty in the minute. These symptoms inducing me to think that hectic was about to be established, I proceeded immediately to lay open the sinus, forming a wound from three to four inches in extent. The bone at my first examination, did not feel rough, but was denuded of its periosteum, and from the dull white appearance it exhibited, I apprehended it was extensively diseased. A light dressing was applied to the wound, and as the boy's bowels were in a costive state, a dose of Glauber salts was directed, with light diet.

9th. The medicine had operated well, affording much relief—I could however observe no material alteration in his condition, the ulcer still discharging copiously.

Having every reason to conclude from the symptoms above enumerated, that hectic was about to be established, I put him on the use of bark and wine—with the elixir of vitriol, (to be used during the remission of fever)—and a nourishing diet.

I think it unnecessary to occupy the time of the society with a minute detail of each day's practice. Suffice it to say, that I was in constant attendance for two weeks, without being able to observe the least amendment, either in my patient's general health, or in the state of the ulcer. On the contrary, I was convinced, from the continued fever, and copious discharge, together with the increasing emaciation, that it was utterly impossible for him to survive without a removal of the leg.

It is true, exfoliation might possibly take place, yet still I considered waiting for such a tedious process (under the present unfavourable circumstances) was tantamount to yielding the patient up to death. What strengthened the opinion I had taken up, was a discovery made in the course of dressing of two sinus's extending up to the tuberosity of the tibia, which I thought communicated with the joint from the puffiness and pain felt on pressure. The tibia too, as far as it was exposed, say four inches, showed no signs of life, although deeply cut into.

Taking all these circumstances into consideration, I suggested to the boy's owner the propriety of an amputation above the knee--the matter was left with myself and father, who from motives of delicacy declined giving an opinion. A medical gentleman met me in consultation, his views of the case not coinciding with my own, the operation was deferred, under the delusive hope that exfoliation might take place.

At the expiration of another fortnight, the boy's situation

was truly deplorable. His fever had assumed the type of a regular marked hectic, and diarrhœa had commenced, the pulse occasionally intermitted, and there was a total loss of appetite—superadded to all these formidable symptoms, the knee of the opposite side became puffy, and considerable pain was experienced on pressure. The boy now complained more of the new action than of the original affection. After stating to the boy's master, the utter impossibility of his being able to survive for many days such complicated disease, and at the same time candidly confessing, that even the efficacy of amputation at this late period was questionable, but that it presented the sole hope of preserving life, I left it entirely with him to decide whether the experiment of amputation should be tried or not. I was desired, after a good deal of debate, to give the poor fellow a chance for his life, and as no time could be lost, the operation was soon performed above the knee, in the presence of Doctors Hening, Skipwith, and my father. The boy bore the operation remarkably well—fortunately for him, little blood was lost—the pulse now became more regular than I had observed it during my attendance; in the course of the night, however, it lost that regularity, and assumed the unfavourable character it had previous to the operation.

As it was of the utmost importance the diseased action of the opposite side, evidently tending to white swelling, should be arrested, I kept up a discharge from the neighbourhood of the joint by blisters, which were dressed with mercurial ointment. About the fifth day, the stump was dressed; it was doing well; the general health of my patient also showed some symptoms of amendment, but in so slight a degree, that I could not flatter myself, (such was his low condition) he could possibly recover. On a subsequent visit, I was very much alarmed at finding the extremity of the femur of a dark colour, and the granulations surrounding it discharging a thin sanies, and at this period I am convinced, if the greatest attention had not been paid to the nourishing course, with a liberal use of bark and wine, the boy must unquestionably have died. From the 1st to the 15th of May, the boy's health gradually improved—the swelling of the knee by this time had entirely subsided. He continued to convalese until the 28th, when he was discharged cured.

As my chief wish is to call the attention of the society to Necrosis, by no means a common occurrence, and as I consider this one of the most complete instances of it on record,

I will detail the appearances exhibited on a dissection of the leg.

The tibia was found entirely deprived of life, except immediately at its articulation with the femur; even there disease existed to a considerable degree, the synovia being offensive, and the crucial ligaments much thickened. The sinuses I have alluded to, had communications with two corresponding openings in the head of the bone: and here let me remark, I never saw more complete destruction of parts; the absorbents were busily employed in carrying off the dead bone, and had proceeded so far as nearly to separate the head from the body of the bone; in other parts of the tibia, they had not been idle. The muscles contiguous to the tibia, had lost the red appearance characterizing them in a healthy state, and were a yellowish and gelatinous mass, with here and there bony nuclei interspersed.

The fibula was sound. Nature not unmindful that it is her duty if she removes one bone, to substitute another, was going on in the re-production of a new tibia—the periosteum, (except at the site of the ulcer, and above where the disease was greatest) had become very much thickened, and of an ossious nature; and this appearance was complete at the lower half of the bone—this casing was very spongy and vascular. I have no doubt if the boy's constitution had been better, and the necrosis partial, she would have (as in many instances she has done) very readily succeeded; but in the present instance, the death was so general, that in her gigantic efforts to relieve herself, she was only hastening her own destruction.

No. 8. *An Inquiry into the forms of Pulmonary Consumption, with remarks on the mode of Treatment in each form.* By B. T. Archer, M. D.

In an inquiry on this subject, I deem it unnecessary to enter into a detailed history of the disease, tracing it from the first period at which it attracted professional attention. to the present day; or to cite the opinions of the learned in the profession, which have, at different periods, received the approbation and concurrence of a large majority of the medical world.

This plan would be too extended for the ordinary limits prescribed to an essay. Suffice it, therefore, to say, that the disease is coeval with the first dawn of science on the pro-

fession of medicine, and that it has received from its votaries a due share of attention. Yet, notwithstanding its antiquity, and the devoted attention it has received, humiliating as the concession may be, we are, nevertheless, constrained to acknowledge, that the mode of cure is still a desideratum in the profession; and must so continue, until the forms of the disease are better understood. I shall therefore commence by avowing the opinion, that pulmonary consumption is of two distinct kinds: first, that form of the disease proceeding from a general affection of the system, locating itself on the lungs; and, secondly, that form of the disease proceeding from local irritation, unconnected, in the commencement, with general affection.

That the operation of the two causes above stated, however dissimilar in their characters, produce effects very similar in their appearance, we will not hesitate to agree. And indeed, that, after the formation of the disease it would be extremely difficult to determine by which of the causes it had been induced; though it must be clear to every one, that the mode of treatment would depend materially on a knowledge of that fact; as, in the one case, our remedies should be directed to the correction of a general affection of the system, and therefore general in their operation; whereas, in the other case, our remedies should be adapted merely to the removal of local irritation, and therefore local in their action. It may be urged in opposition to this view of the subject, that pulmonary consumption, whatever its form or character, is uniformly induced by predisposing debility. For the correctness of this assertion, we have the authority of a gentleman, who has been more distinguished in the profession, than, perhaps, any other individual in the country.* A gentleman, for whose opinions I feel a respect bordering on veneration. But when we recollect the circumstances under which this opinion was promulgated, splendid as were the powers of the mind that framed it, we can readily conceive how it may have been misled, or deluded. Having adopted new principles in the science of medicine, with sufficient influence of character, and energy of mind to give them diffusion; feeling, as he himself acknowledged, that sort of attachment for his theory, which a parent feels for his child, we can easily imagine that, under the influence of such emotions, all the energies of a gigantic mind were employed to bend every circumstance to its purpose. Thus I believe many errors

* Professor Rush.

were innocently committed, and this, perhaps, not the least conspicuous among them.

That one form of consumption is induced by predisposing debility, I will not hesitate a moment to acknowledge; the evidences of this fact are too numerous and clear to admit of doubt. That in this form the indication would be, to remove the remote and existing causes, by remedies calculated to produce a general effect, such as blood-letting, mercurial action, arsenic, emetics, &c. &c. In the other form, the remedies above cited would, in all probability, aggravate the symptoms, by uniting with previous local irritation, general debility; thus inducing reaction, which had not before taken place.

As evidence of the existence of two forms of pulmonary disease, the following facts are submitted. 1st. The opposite effects of the same remedies in the hands of different physicians, and sometimes in the hands of the same physician, in different cases. 2dly, That persons apparently in good health, enjoying in a high degree all the animal powers, are first apprized of pulmonary derangement, by an attack of hemoptysis, unattended with general plethora of the blood vessels. A case of this sort recently fell under my observation, in which the usual remedy of blood-letting was freely used in the commencement of the attack; evidently, I thought afterwards, with injury to the patient; its effect being rather to increase, than to diminish local plethora. 3d, That tubercles, and, indeed, an abscess, have sometimes existed in the lungs, without cough, pain, or fever, for some time after their formation, or a knowledge on the part of the patient, of previous diseased action. This form of the disease, I should regard as perfectly local in its commencement, as much so as an ulcer produced on the surface, by the application of cautery.

And, lastly, that consumption is frequently produced by mal-conformation of the breast, is, I believe, a fact universally acknowledged. That this mal-conformation may induce debility, by difficult respiration, or obstructed circulation, is perhaps an opinion equally well founded; but that we cannot in this case call in debility as the cause of the disease, without violating one of the established rules of philosophising, which directs, that we shall not call in a greater number of causes to explain an effect, than are sufficient for the purpose. The mal-conformation, therefore, I regard as perfectly sufficient to explain the effect, without calling to its aid additional causes.

Having advanced thus far towards the establishment of my

first proposition, I will now offer a few suggestions as to the mode of treating each form of the disease. And first, of that form induced by predisposing debility, and consequently connected with a general affection of the system. The indication in this case would be to remove the cause of the disease, *(debility.)* The mode of effecting this, should depend on the degree of re-action induced by the operative cause; if great, the ordinary remedies for reducing inordinate action should be adopted. Among these, blood-letting may be regarded as the most efficient, from its immediate action on the arterial system, and its secondary operation on the stomach and surface, exciting nausea in one, and moisture on the other. Thus producing general relaxation, until the diurnal period for renewed re-action, when it will be necessary to repeat the remedy for a succession of days, until the re-action induced by debility is nearly overcome. To remove effectually the re-action by this remedy, would hazard a subsequent state of debility, more alarming than that produced by the first cause. The second remedy in point of force, is mercurial medicines, these, by exciting a new diseased action, perfectly under the control of the practitioner, may be continued for any length of time, with the hope, when they are withdrawn, of having renewed healthy associations. The curative operations of arsenic, may I think be explained on the same principles—the third remedy on which we may rely, is emetics, either in full or nauseating doses; among this class of medicines, the one on which I would principally rely, is the sulphat of zinc —from its prompt and efficient action on the stomach, combined with an astringent operation. The nauseant most uniform in its effects, is a due combination of opium and ipecac. From the sulphat of zinc, and opium and ipecac., we may be availed of all the advantages to be derived from this class of medicines. Under the fourth enumeration of remedies, I shall place digitalis and Prussic acid; each article having in turn acquired considerable celebrity in the cure of consumption. The contradictory authority, however, on which their reputation rests, render them extremely doubtful remedies. The mucilaginous preparations, demulcent teas, and syrups, (too tedious to enumerate,) may be used in the progress of the disease as palliatives. After the correction of re-action, by the operation of one, or all, of the remedies above cited, a vigorous tonic course should be adopted. Among the medicinal tonics, nitric acid promises the greatest advantage. The tonic effects of exercise in all the forms and gradations recommended by Professor Rush, should receive strict atten-

tion. I will now proceed briefly to state the few and simple remedies, indicated in that form of the disease, produced by local irritation. These are first, issues and blisters; and second, cooling purgative medicines. The mode in which the first class of remedies, produce an effect, is sufficiently understood; by exciting on the surface a degree of irritation greater than that previously existing, and continuing it for any length of time without the slightest risk to the patient, we are enabled to concentrate irritation in a point perfectly under our management; the cooling purgatives facilitating in some degree the desired translation of irritation. That consumptions of alarming character, have been cured by external applications, we have the authority of many distinguished physicians. That caustic issues are the most effectual of these remedies, we have the authority of a distinguished physician of Virginia,* who relates two cases of successful treatment by this remedy—In all local affections of the lungs, I should rely confidently, on the success of the above remedies.

No. 9. *Remarks on Anomalous Bilious Fever, by* John Dove, M. D.

There is perhaps no subject more interesting to the experienced, or more embarrassing to the youthful practitioner, than the anomalous forms of fever generally, but more especially those which occur in sections of country where bilious fevers are most predominant; it is here that the fallacy of nosology is most apparent, and it is here that the invaluable precepts of our illustrious countryman shine conspicuous; thus when you come to examine any treatise upon bilious fever, you will find a long enumeration of symptoms laid down with great pains and accuracy, beginning at the cold and passing through the hot to the sweating stage, which I will not detain you to hear; these are generally drawn from the most aggravated forms, and combine all the features which may have appeared in one or two hundred cases.

When the young practitioner, fortified as he thinks with a perfect recollection of all those symptoms, is called to a patient labouring under fever, but who has had no rigor, furred tongue or headache, he concludes that this cannot be

* Professor Rogers, of Williamsburg.

bilious fever, and perhaps proceeds with such remedies as are entirely injurious, but which seem to be indicated.

I would not be understood to allude to the anomalous symptoms which frequently arise during the progress of a fever, but distinctly to bilious fever assuming entirely, or nearly so, the form of other diseases; almost all authors on fever speak of anomalous symptoms, a late one, Johnson, on Tropical Climates, is replete with observations of this kind, and the very observant Sydenham in his laws regulating epidemics notices them frequently; the late Dr. Rush used to impress upon his class, the well known fact that epidemics caused all endemics of every country to wear their livery for a time; thus the typhus epidemic of 1815, which prevailed through the United States caused the endemics of each state to wear its livery, so far as to influence the practice in a most remarkable manner, and to this fact may be referred without doubt the great discrepancy of opinion among medical men of equal eminence when writing or speaking of this disease as an epidemic.

The great axiom of Rush, that "disease is an unit and the pulse its nosometer," is perhaps more completely applicable to bilious fever, than at first sight may be imagined, and were I to utter an axiom in medicine, it would be that "bilious fever is an unit and the tongue its nosometer;" for the pulse much more frequently refuses to sympathize with fever generally, than the tongue does with this form, and if unwariness were tolerable under any circumstances in a practitioner, it would be, as I think excusable where the tongue did not sympathize with bilious fever; yet such a case I have seen, but always with great alarm.

The reputed success of apparently opposite remedies in this form of disease is no doubt referrable to this fact, and I have little question, that the invaluable work of Hamilton on Purgatives, has done serious mischief, by not observing that many of the diseases which were cured by him with these potent engines were only misplaced forms of this same disease; thus rheumatism he avers may be cured by purgatives; when it is a misplaced form of fever, I have no doubt it may—nay, I have seen such a case, but when it occurs as an idiopathic or primary disease, I do not believe it ever was or can be:—And here I would observe, that I regret exceedingly having lost the notes of an extremely interesting case of symptomatic rheumatism, which my partner, Dr. George Cabell, saw with me; the prominent symptom of an exacerbation of bilious intermittent fever, which this patient had, was a recurrence

of rheumatism of the arms at the same hour of every second day, which continued about eighteen hours each time, and which was cured by an emetic, cathartics, and the bark in full doses.

But the vicarious resources of nature are by no means confined to the joints, muscles, glands or excretory ducts; the blood vessels themselves, as I shall presently show you, sometimes take on this anomalous action, and I am inclined to think more frequently than is noticed, in the shape of hemorrhages of the various kinds.

On the second day of May last, I was called on at my office by Mr. Richard Swim, a turner by trade, of middling stature, spare form, and regular habits, about thirty-five years of age, he had been occasionally troubled with epistaxis during the two preceding days, and which was now quite urgent, he complained of no pain, fever, or distress of any kind, his tongue was slightly furred, his pulse healthy; I gave him a drastic cathartic, and ordered a styptic application of a solution of the sulphate of copper.

On the 3d, about the same hour, I was sent for in great haste, and found him bleeding very freely from the nose, the purgative had acted freely, his tongue was more furred, no febrile excitement at all, pulse natural; I applied a blister to the back of his neck, ordered a styptic solution of crocus martis, and gave him the sugar of lead, opium and ipecac, directing the purgative to be repeated.

On the 4th, in the morning, he informed me that about seven years before he had the yellow fever in Charleston, (S. C.) and that on the 7th day his physician visited him and gave him up to die, that in an hour after a profuse epistaxis took place and he recovered immediately; that he had been since afflicted with bilious fever frequently, and each attack had been attended with epistaxis, which the doctors had in vain tried to stop with astringents.

Being now satisfied that this was a case of misplaced form of bilious fever; I left a full dose of emetic tartar to be taken an hour before the time for the discharge to become great; for it will be recollected that some discharge continued all the time; after the operation of the emetic he was to take a large dose of calomel, worked off with jalap; this day the bleeding did not return in any great quantity; the emetic having previously brought off a copious discharge of highly acrid bile, which was kept up by the calomel.

On the 5th, in the morning, the epistaxis has nearly ceased, his tongue is still furred, though much cleaner; continue the

purgative action; at 4 o'clock P. M. I was sent for in great haste saying Mr. Swim is "certainly bleeding to death;" I found him bleeding freely, and directed him to swallow a large draught of strong salt and water, a remedy which I have found very efficacious in common epistaxis, my patient was immediately seized with a most profuse vomiting of yellow bile, which continued near an hour, the hemorrhage in the mean time ceased: being now fully convinced of the nature of the attack, and believing that another paroxysm would prove fatal, I told him I considered the attack ague and fever, and that all depended on my being able to stop the next fit; I then directed him to take ℥j. of sausford bark, with ℥ss. cremor tartar every hour, in a wine glass full of port wine, during the day and night. My prescription was rigidly followed, and the next day, instead of the epistaxis, he was seized at the regular hour with an ague of severe form, followed by high fever which terminated in a most profuse sweat, but no bleeding recurred.

At this visit I exhibited a purgative, and on the 7th recommended the bark; at the usual hour he had a slight fever, continued the bark next day, and in a few days my patient was discharged cured. My partner, Dr. Cabell, visited him with me on the 6th, and had this statement in nearly all its particulars from the patient; he agreed with me in considering it a case of bilious intermittent fever.

In this case, had much time been lost in administering styptics and applying astringents for idiopathic epistaxis, the patient must have died; yet will any one have the hardihood to say that emetics and purges will cure epistaxis or common hemorrhage from the nose, because they have done so here?—Or can any man say this was not bilious fever disguised, because nine-tenths of the symptoms laid down in books were wanting?—Surely not.

I am not anxious to retain the name of fever, or to quibble about words or nomenclature, but wish to be understood to mean by bilious fever a certain derangement of the animal economy, attended with bilious action, or disorder of the liver.

It is worth mentioning, that the hemorrhage did not appear to come from any one vessel in the nose, but to be a weeping, or flow from the whole internal surface, though evidently connected with a spontaneous rupture of some artery or arteries, as the blood discharged presented all the appearances of arterial.

Thus, gentlemen, I have endeavoured to show, that the

blood vessels may, and do perform vicarious action for bilious fever, and I think the fact so palpably proven by the symptoms and consequences above enumerated, that, at any rate, we should be more guarded in our inquiries after the cause of hemorrhage than we usually are. And why, I would ask, may not the blood vessels perform this action or duty? Even reasoning, a priori, the stomach, it is well known, and now universally admitted, does perform vicarious action with the uterus; by what train of causes, or modus operandi, no man, I presume, will undertake to say he knows, but, like many other phenomena, he must be content with the fact. Congestion, the frequently reputed cause, will not account for it; for that cause alone, would as soon force it from the uterine region, as from the epigastric; nay, one would think, more probably, as the parts are more vascular, and much more fitted by habit to the discharge of that fluid.

Had this young man been plethoric, perhaps the advocates for the use of the lancet in bilious fever might say, that the discharge was from the want of that remedy, and purely accidental, perhaps critical; but as he was not, and as the hemorrhage was repeated with so much accuracy as to periodical recurrence, I presume it will be admitted, that the discharge can only be viewed as a bilious fever, of anomalous kind; more particularly, when we recollect that among authors upon the subject of bilious and yellow fever, it is by no means uncommon to see accounts of hemorrhages taking place, sometimes in the beginning, sometimes in the progress, but more frequently in the decline, from the nose, stomach, and bowels. This is more common in yellow fever, which I consider only as a more aggravated form of bilious fever; but in this opinion, by way of digression, I find a late author in the Medical and Physical Journal of Philadelphia, (Dr. John Stearns,) differs widely from me, though his reasons, which to him seem so conclusive, granting them true, which I do not, to me seem any thing else.

In consequence of the high authority of that journal, and in compliment to it, as well as to elicit truth, I will detain you a moment, to hear some of his diagnostic symptoms, between bilious and yellow fever.

After saying the diagnostic pulse of bilious fever is tense, full, and generally regular, which I presume I need not comment on, he says, 2d, "Except the usual flush of ordinary fever, the bilious fever exhibits no unnatural change in the countenance, or in the appearance of the eyes." If he alludes to the case of disease which I have just laid down, or such a one, he is

right; but I think you will all bear testimony with me, that the eyes are, as often as ninety cases in an hundred of bilious fever, of a yellow, listless, watery appearance, and the whole countenance, particularly the forehead, frequently of a slight yellow appearance; and thus this diagnostic fails in ninety out of an hundred times.

3d, In bilious fever "the skin is hot, dry, and elastic." Who has not frequently seen the reverse of this? Surely every practitioner has seen the temperature scarcely increased at all. Such a case I have now under treatment. Again, how often is your patient continually immersed in general perspiration?

4th, In bilious fever, "the debility of the muscular system *always* increases as the disease approaches a fatal termination, and the patient is often unable to move, while the arterial action continues stronger than natural." Have we not often seen our patients, almost in articulo mortis, rise from the sick couch, and walk across the chamber, sometimes resisting the strength of a healthy attendant? A case of this kind I lately had under treatment, in which the patient, a few hours before death, got up, and in defiance of his nurse, walked out into the street.

5th, In bilious fever, "the intestines are excited by ordinary cathartics." This is so egregious an error, that it need only be mentioned to be refuted. And indeed, if I might judge from the symptoms of bilious fever laid down by this writer, who certainly ought to have been well informed before writing, I should say that he had never seen a case of bilious fever in his whole practice, or if he has, what is still more unpardonable, he has grossly mistated it. His diagnostic symptoms of yellow fever are more correctly drawn, and, in most particulars, resemble our bilious fevers very closely.

In a subject of such immense importance to the community, it would seem, however, that something more than orthodox assertion should be used, particularly when such assertions reach us in a shape the most imposing, and through a channel, whose sole object should be, to inform and instruct; for it will be remembered that Dr. Stearns set out to show the causes which have retarded the progress of medical science, and among the number, by no means the least efficient, as he thinks, shows medical books to be one, owing to their fallacy of statement, and theory upon those statements.

In this paper, the identity of the cause and seat of the two diseases, is very prudently kept out of view, for in the history

of diseases whose causes and seats are known to be very nearly, if not entirely the same, it would require great ingenuity to prove a single diagnostic difference.

Were we, however, to set down a certain train of febrile symptoms, as diagnostic of bilious fever, and say that every departure from it is something else, where, in the name of common sense, should we end with the innumerable varieties of bilious diseases, which daily occur, and to which, of course, it is thereby intended that some new remedy should be adapted; for at last, it is with a view to facilitate the curative indications, among the youthful class of our profession, that such distinctions in disease generally, are at all necessary or useful.

The gigantic mind of the immortal Rush, has inflicted a fatal blow upon this many-headed monster, which it will take ages of error and infatuation for novelty, to regenerate; as well, said he, "might you break in pieces the loadstone, and call each from its exterior shape by a different name, as subdivide fever, and call each after some anomalous symptom;" this assertion, gentlemen, I hold to be peculiarly true of bilious fever, and I hope will never be effaced from the memory of his class while the name of Rush holds its proud seat among the devotees to medical science.

No. 10. *An Inquiry into the functions of the Liver, in preserving the health, and removing the diseases of the animal economy.* By Robert Briggs, M. D.

The liver is viewed by the great majority of physiologists, as merely an auxiliary organ to the stomach and intestines, in performing the work of digestion. A variety of considerations lead to the conviction, that its primary operations are of a very different kind; and that if it contributes at all, directly to the digestive process, this is but a secondary effect.

When we take into consideration the magnitude of the liver, its situation, its connections sympathetically with every organ of the body, its contiguity to, and direct connection with, what may justly be considered the great centre of the involuntary nervous system, its large supply of blood from venous sources, and this derived almost entirely from digestive organs, and the comparatively unimportant effects produced by the fluid secreted, we shall be led at least to doubt, whether this great viscus does not perform other offices, more essential to the preservation of life than those generally attributed to it.

The great size of this viscus in the fœtus, when compared with every other, the secretion of bile at a time, and under circumstances which can require none to be present in the intestines for the purposes of digestion, the quantities of this fluid poured out at all periods of life, after the digestive apparatus has been called into action; when not at all required in consequence of their being no materials present to be digested, and when there are materials, if the system is labouring under disease, bile not producing the effect of changing them into that state necessary to afford nutrition; chyle, the form to which all nutritious articles must be reduced, before they enter the lacteals, being as readily and effectually formed when the biliary duct is completely obstructed, as when most free, urges upon us the conviction, that this organ is designed for other purposes.

The doctrine which is attempted to be maintained in this essay, is, that the liver acts the part of a guardian to the general system, preserving the purity of the circulating fluids in a state of health, and freeing them from the contamination induced by disease.

The placenta performs for the fœtus the offices of both respiratory and digestive organs. The blood sent by the circulatory system of the fœtus, is freed by the placenta from certain impurities, as it is by the lungs subsequent to birth. Nutritious matters are also furnished by the placenta, mingled with the blood returned by the umbilical vein. Far the greater part of this blood circulates through the liver, in the same manner which the blood from the digestive organs does after birth, when the supplies of digested materials furnished by the placenta are cut off, and now supplied by the stomach and intestines. M. Magendie has established the fact, that veins perform the office of absorbents. The branches which when united form the vena porta, arise from the surface of the digestive organs, and must necessarily carry blood of qualities very different from any other portion of the vascular system. The lacteals all pass through glands before they reach the thoracic duct; the fluids which they carry undergo a decided change in passing these bodies. The lymphatics are believed by Dr. Chapman to convey matters formed by the waste of the body, and unfit for farther use. The fluids which they carry, find their way out of the system by no direct route, but pass again into the general circulation.

If the fluids conveyed by the lymphatics, can no longer serve any valuable purpose in the body, they must be thrown off by some emunctory, or they would soon produce disease.

Portions of these fluids may be thrown off in the form of perspiration, urine, &c., but for reasons which will be presently assigned, it is believed the larger part pass out elaborated into the form of bile.

In the continued wear of the various machinery of the living body, large quantities of matter worn out and unfit for the purposes of animal life must be thrown off. By what organ is the office performed, of separating these matters from the circulating mass, and eliminating them from the system? When the body has acquired its full growth, the quantity daily thrown off, must be equal to the amount of nutrition taken in, otherwise a continued increase must progressively go on, ad infinitum. We know, that the several excretions taken together, amount to about the quantity of food and drink taken in, and bile is known to constitute a very considerable part of the fæces in a state of health. The skin, the kidneys, the mucus follicles of the intestines, &c., secrete fluids composed principally of oxygen and hydrogen in the form of water, holding certain saline substances in solution: whereas bile contains the greater number of the salts held in solution, by the more fluid secretions, in addition to which, the substance contributing most largely to its composition is carbon, one of the principal ingredients in animal solids.

The humoral pathologists laboured to account for every phenomenon exhibited in disease, by changes in the fluids—a number of circumstances remained unexplained upon these principles, a more rational theory was brought into view by admitting the solids to a participation in the production of morbid action. It is a universal disposition in the mind of man, to go to extremes with every new and favourite theory. The system ascribing the production of disease to the action of the solids, ultimately usurped entirely the place of humoral change, and the latter when admitted, was viewed as but the consequence of the former, but most frequently denied to exist at all.

Within comparatively a few years, however, the humoral doctrines have been again called into requisition, and from the wreck of former opinions, a newly modified system has been reared.

That there are changes produced in the circulating fluids of the body labouring under disease, cannot be denied; whether we admit them as cause or effect, is not material to the present inquiry.

The sentiment insisted on in this essay is, that the liver by its increased secretion, frees the general system from some

offending matter; or by its own action in forming its fluids, changes the action of the nervous and vascular systems, from disease to health. What it is that constitutes disease, or what the proximate cause is, we know not—why blood-letting tends to reduce diseased action in inflammatory affections, or why certain stimulants have the effect of restoring a more salutary condition, in advanced stages of typhus, or indeed why any particular agent produces an effect peculiar to itself, and different from others, we know as little. But from experience and observation, it has been ascertained, that such is the law of the animal economy, and such is all we know of it. Why an increased hepatic secretion should have the effect of relieving the system at large, or any particular organ from that condition which constitutes a departure from healthy action, we are equally ignorant. That this however, is the fact, will now be attempted to be shown.

In fevers of every character, we find the action of all the other glands greatly diminished, some entirely suspended. The salivary glands cease in a great degree to pour out their fluid, the gastric juice is no longer supplied by the coats of the stomach, the bowels become torpid, the quantity of urine lessened, the skin dry and parched, while the liver is found to assume an increased action, and bile is poured out in larger quantities.

When the liver is in a diseased situation itself, at the time the general system is invaded by disease, not produced by the hepatic affection, or any individual organ becomes deranged in its functions, or the diseased action of the general system is such as to suspend the functions of the liver; those diseases are found under such circumstances much more difficult to control, and impossible to be removed, until the liver is so far restored to a healthful condition, as to be able to resume its wonted action.

In yellow fever, one diagnostic symptom is the absence of bile, the appearance of which is viewed as a favourable circumstance.

In cholera morbus and dysentery, no amendment is discoverable until bile is freely formed.

Cholera infantum belongs to the general class of febrile diseases, the remedies for which are those calculated to correct the vices of the biliary functions.

In dyspepsia, the liver is found in every instance to be excited into increased action. In many cases of this disease, the stomach is relieved from its disorder, but the task imposed upon the liver is such, that it is often goaded into disease

itself. If no remedial course is adopted, to relieve it from its increased burthens, structural derangement is at length produced, and it consequently ceases to carry on that process, by which the system was preserved from destruction. Under these circumstances, the stomach does not usually return to that condition from which it had been relieved by the vicarious office of the liver, but assumes a new diseased action, by which an effort is made (if the expression may be allowed) to personate the liver relatively to the general system, in the secretion of bile. It has been ascertained by Sir W. Phillip, that the green watery fluid ejected from the stomachs of dyspeptics, who have been long diseased, is not a secretion of the liver, but an imitative process of the stomach itself.

In hydrocephalus, whether we view the state of the liver as cause or effect, the reasoning still holds good, as to the essential necessity of the biliary secretion, to the restoration of health. Whatever mode of practice may be adopted, until the biliary secretion is excited to increased action, no mitigation of cerebral disease is ever evinced.

Obstinate ulcers of the surface, indurated tumours affecting the breasts of females, and exhibiting all the threatening characters of incipient scirrhus; are frequently removed by correcting the state of the hepatic functions.

In chlorosis, hysteria, hypochondria, chorea, &c., diseases belonging to the class of nervous affections, the principle governing the remedial course is still the same. Whatever may be the plan of treatment, until the liver is brought to assume an action sufficiently vigorous to free the general system from the offending material, or that modification of action which constitutes disease, no benefit is derived. That a state of fluids exists in these and many other diseases, differing from that presented in health; or a modification of action tantamount in effect, is inferible from the character of the biliary secretions, when the hepatic functions are brought to assume the curative process. The fluids discharged from the biliary ducts, at first bear but a distant resemblance to healthy bile, and the patient is found to improve, in proportion to the approach of this fluid to a healthy character.

The increased quantities of bile thrown out at the termination of a paroxysm of intermittent, has been attempted to be accounted for, by the quantities of blood driven to the interior organs, by the spasms affecting the skin. This position cannot be maintained.

Under this view of the subject, in continued fevers, after

all coldness of surface ceases, and universal increase of temperature pervades the exteriors, the quantities of bile should be greatly reduced; instead of being increased, as is almost invariably the case.

The application of cold water to the skin at the height of the febrile paroxysm, should have the effect in some degree, of producing the same result as the cold stage of an intermittent, by driving the circulating blood in greater quantities to the interior, and thus increasing the quantity of bile. That which is contrary to all this takes place. In proportion as the heat is reduced, we find the quantity of bile lessened. In this process the heat is reduced, and consequently a morbid stimulus is removed, which when present, although generated by the action of the system itself, re-acts on that system and increases the violence of disordered action. In proportion as this violent action is diminished by the abstraction of any stimulating agent, the demand upon the liver as an emunctory is reduced.

Another theory attempts to account for the increased flow of bile, particularly in idiopathic fevers, by supposing the miasmatic or effluvial poison, to be taken into the stomach, and there first producing gastric irritation, that the liver is called into action sympathetically, and increased quantities of bile the consequence.

Another mode of accounting for increased quantities of bile is by considering this as the result of general increased action of the vascular system, and of the liver as a part of that whole.

If the first of these suppositions were correct, we should as the first effect of the morbid poison, have an increased secretion of gastric juice, hunger, vigorous and rapid digestion: the reverse of all which is found to exist.

Under the second hypothesis, the secretion of every glandular viscus of the body should be increased. We should be justified under this theory, in expecting during the existence of acute fever, a display of increased mental energies, a larger flow of saliva, greater quantities of gastric juice, consequently keen appetite, larger quantities of bile; and if this contributed as essentially to the process of digestion as is generally conceived, food taken in obedience to the appetite, created by an abundance of gastric juice, would be rapidly converted into chyle, and conveyed into the system; an increased flow of urine would be the consequence of increased action of the kidneys, a free perspiration from the cutaneous vessels, and

in short, we should have every febrile patient, thriving and fattening in a direct ratio to the violence of his disease.

In different diseases, and different stages of the same disease, the character of the hepatic secretion is discovered widely to differ; under all, or any of which circumstances, no local affection of the liver itself is found to exist.

How shall we account for these various products of the same organ, while itself is not diseased; but by supposing its action accommodated to the necessities of the system over which it exerts a protecting and delivering power.

In forming its secretions, whether it derives the materials from offending matters circulating with the fluids of the body, or whether it is by changing the ordinary state of the fluids into something different, so as to act salutarily on a morbid condition of the solids, and thus effect a restorative change, or by what other mode the result is produced, remains yet to be ascertained. In different diseases that the hepatic secretion is widely variant, is a truth known to every tyro.

We find the liver throwing out various fluids from those transparent, colourless, and tasteless, through all the varieties of yellow, green, blue, black, and red: these possessing every degree of consistency from the fluidity of water, to the firmness of coagulated blood; and lastly blood itself. The blood discharged from this organ is viewed as a secretion. Among other reasons for this opinion, are those that arise from the difficulty of conceiving by what channels it obtains egression in such quantities, than by those which give exit to bile in health, and its destitution of the properties of coagulation.

Febrile patients are found not unfrequently labouring under diarrhœa; when the matter discharged is principally bile, the disease soon ceases, and health is restored: (unless the affection is kept up by obstruction of the mesenteric glands.) But where no bile is secreted, whatever quantities of fluid may be thrown out, it is unproductive of good.

The observation of every practitioner must have furnished him with repeated instances, of the merely transient effects, produced on febrile action, by blood-letting, purgatives, and the whole routine of evacuants, so long as any degree of torpor pervaded the functions of the liver.

In almost all cases of protracted fever, the liver is found to a greater or less degree, in the one or the other of two very opposite states, that of torpor or inflammation. In either condition, we find the disease obstinately running on, until

recourse is had to those agents which are known to exert something like a specific influence over the diseased actions of this viscus. Amid this defiance of every other order of remedy, a few grains of some mercurial preparation are found to act like a charm, in restoring order and tranquillity to the system.

ART. III. *Observations on the transplantation of Teeth, which tend to show the impossibility of the success of that Operation: supported by a new theory.* By JAMES GARDETTE, Dentist.

I HAD, a considerable time since, determined to write on the subject of the transplantation of teeth, from the mouth of a living person into that of another; but my great occupations, and the diffidence I always had in writing English for publication, have prevented me from attempting it; and it is probable I should never have done it, had I not been encouraged by my friend, Dr. Mease, who, after a conversation on that subject, lent me the first volume of the Memoirs of the Medical Society of London, in which several cases of disease succeeding the transplantation of teeth are published, by J. C. Lettsom, M. D., &c.; one of which happened in August, 1785. I read the articles with great attention, and found I was acquainted with the first case, which had been related to me by the gentleman, J. Y., now a citizen of Philadelphia, in whom it had occurred a few years before.

The reading of those articles, and the observations of my friend, Dr. Mease, who is very much alive to the dissemination of useful knowledge, determined me to write this paper, and at the same time to endeavour to prove the impossibility of transplanting teeth from one mouth into another, with the success expected and promised by the operator, viz. that the tooth transplanted will remain firm, and as useful as the tooth which had originally grown in the jaw.

I shall preface this paper by informing my readers that I arrived in Philadelphia, in June, 1784, and began to practise my profession; and that Mr. Lemayeur, with the reputation of an eminent dentist, had transplanted one hundred and seventy teeth in this city, in the course of the winter of the years 1785 and 1786, as he told me himself, at Baltimore, in the fall of the last mentioned year; and that, of all those transplanted teeth, not one succeeded! Some became firm, and

lasted, more or less so, for one or two years, in the sockets in which they had been inserted; but those cases were very rare. In the course of my practice, after that time, I had occasion to extract at least fifty of these transplanted teeth; most of them without an instrument, with my fingers only, and to replace them by artificial teeth. Many accidents occurred to the transplanted teeth, while they were growing firm, and some never got firmly fixed in the sockets at all. I shall now relate some cases of that nature, which happened to teeth transplanted by Mr. Lemayeur, which, I dare say, will be recollected by some persons now living in this city, and perhaps by relations of the persons who were operated on at the time.

Mrs. A. W., a lady of great respectability, had several, I believe *three* front incisors of the upper jaw transplanted; after suffering for a considerable time, the transplanted teeth not becoming firm, she was obliged to have them extracted, and artificial teeth replaced in their room.

Miss W., a young lady at boarding school, had the four upper front incisors attempted to be transplanted, but they never became firm; the gums were so inflamed and ulcerated, that the disease was communicated to the lip, so as to form a complete adhesion with it; they were separated by scarification, but the adhesion of the gum and lip could not be prevented, until the transplanted teeth were extracted; which, being done, the lip and gums perfectly cicatrized in a short time; the space was then filled with artificial teeth.

I was informed that about the same time, a young lady of New York, Miss S., had a large front incisor transplanted in the upper jaw, which produced a disease, judged by the physicians who attended her, to be the *lues venerea.* This young lady was so affected by the disease that, notwithstanding all the medical aid given her, her health declined, and after considerable suffering of mind and body, she died.

I was also informed, that a Mr. T., of Virginia, had a front upper incisor transplanted about the year 1790, by the same dentist, the exact time not being remembered, which occasioned much inflammation in the gums and eyes. After some time, the *ophthalmia* became severe, and other symptoms justified the opinion, that the *lues venerea* had been introduced into the system by the transplanted tooth, which, no doubt, was taken from an unsound subject. I was informed at the time, that Mr. T. had lost his sight, and that after lingering for some considerable time, he died.

Mr. W. H., of Philadelphia, had three upper front incisors transplanted in London, in the year 1784, or 1785, un-

der the superintendance of the distinguished surgeon, John Hunter; the operation was performed with all the care and skill possible, and the teeth became firm in a short time, without any accident of importance. I saw the gentleman in this city about five years after the transplantation of the teeth, which at that time were somewhat loose. He consulted me as to the cause of the looseness of the transplanted teeth. On examining his mouth, I found that the teeth of the under jaw directly opposite the transplanted ones, struck against them on their internal surface, and I judged that the continued shock occasioned by the under teeth, was the real cause of their looseness; but that the original cause of the under teeth touching the upper, was the inflammation of the gums of the transplanted teeth, which caused their dropping down, and thus to meet the under incisors opposite to them. In order to remedy this inconvenience, I proposed to shorten the under teeth, which I did with a file; I then advised the gentleman to make use of an astringent wash to brace the gums of the transplanted teeth, which were inflamed, and somewhat spongy; my prescription was followed, and the teeth became firm in the course of two weeks. I did not see the gentleman's mouth after this for a considerable time, as he lived out of the city. But having had occasion to see him some years after the time I attended him, I perceived he was without the transplanted teeth, which he had never replaced.

Of all the transplanted teeth that I ever saw, or heard of, none have lasted so long as those transplanted in the mouth of Mr. W. H.; for they remained very firm for about five or six years, and lasted about as long in a loose state, which increased until the teeth either dropped out, or the gentleman extracted them himself with his own fingers; for I am persuaded they were not extracted by a dentist.

None of the teeth transplanted by Mr. Lemayeur, in Philadelphia, remained firm two years, and in two or three cases which I have seen of teeth transplanted by other dentists, they did not remain firm one year.

Mrs. J. P. had a large incisor of the upper jaw transplanted in London, also under the care of the celebrated John Hunter, in the year 1780, or thereabout, (the time, which was mentioned to me, not being remembered,) the tooth became firmly fixed in a short time; but about a year after its transplantation, a small discharge of matter was perceived issuing from the under edge of the gum, on the left side of the transplanted tooth; but this was not much regarded at the time, being very trifling. The daily attention paid the

teeth, by washing and brushing them, prevented the lady from taking notice of its progress for some years.

Having determined to leave London and come to Philadelphia. after the peace of 1783; she had, a few days before her departure, her mouth examined by her dentist, who readily found that a *fistula* was the result of the continual issuing of a small quantity of pus from the socket of the transplanted tooth. It was then judged by him, that the tooth could not remain long in its place; he advised the lady to have a tooth prepared,* that could be easily fixed in the place of the transplanted tooth by a dentist, should there be one in Philadelphia, (which, it appears, was much doubted at the time in London.) After her arrival in this city, Mrs. P. consulted Dr. W. Shippen, who, after examining her mouth, determined that the transplanted tooth should be extracted. The doctor sent for, and asked me if that was not my opinion; after examining and probing that part of the socket which could be reached with a probe, I found that the left side of the root of the tooth, as also the socket, were completely decayed to the extremity of the root, which was perfectly adherent to the socket on the right side, the tooth being still very firmly fixed, notwithstanding the existing caries.

I told Dr. Shippen, that the tooth ought to be extracted, in order to cure or dry up the *fistula*. But there was some difficulty in extracting the tooth without breaking that part of the *alveola*, which was completely ossified with the right side of the root; and which I thought I could avoid by means of an instrument which I would cause to be made by our old and only cutler, Mr. Schively, and which I described to the doctor as follows, viz.: The blade in the form of a narrow straight scalpel, thin, and very sharp pointed. After having informed the Dr. of my intended manner of performing the operation, he approved of it. At the time fixed by the lady, I operated in the presence of Dr. Shippen and a gentleman, a friend of the family, (Mr. John Mifflin,) in the following manner. I separated the adherent plate of the socket from the root of the tooth with my sharp pointed instrument with all possible care, in the space of about two minutes; I then removed the tooth with a straight forceps, with the greatest ease imaginable. The exfoliation of that part of the socket, which required it, and the cicatrizing of the gums, required nearly a month,

* It was a porcelain tooth, made by Dubois Dechman, a French dentist in London, who first invented the manner of making artificial teeth out of porcelain, and which has been so much improved since by several dentists, and particularly by *Fonzi*, an Italian dentist and chemist at Paris.

when I replaced a natural tooth, mounted on a gold plate, after the mode which I had invented about that time; this tooth resembled so perfectly the large *incisor* which remained, that no person could perceive the difference.

The transplanted tooth being examined after extraction, it was found that one half of the root had been destroyed by caries, longitudinally to its extremity, which proved the absolute necessity of its removal.

It is possible that the Dentist who transplanted the tooth, finding the root of that tooth too big, filed off some of its thickness, (as I have heard of that being done sometimes) to let it go easily in the socket; the periosteum having been removed, the root could not adhere to the *alveola* on that side; and that may have occasioned the formation and of course, the emission of the purulent discharge spoken of.

In order to establish my theory, I shall cite some cases which have occurred to me in the course of my practice; which will prove that a diseased tooth taken from its socket, the cavity in it plugged, and the tooth replaced in the same socket, will become in the course of ten or fifteen days, as firmly and solidly fixed as it was before the extraction, and last for a great number of years, and sometimes during life. This is certainly a tooth transplanted to all intents and purposes, but in the same socket from which it was extracted. If therefore another tooth could have been found, the root of which was exactly of the same length, size and form, it might have been placed in the socket of the tooth just spoken of; and it would certainly have become as firm, and have lasted as long as the tooth which had grown in that socket.

I have frequently, partially extracted and returned to their sockets, small and large molares, which had been very painful; after having cut the gum on the side opposite to that on which I intended the tooth to fall in partially extracting it. The purpose of this operation is to separate or rend the nerve asunder, so as to prevent the tooth from giving pain in future; the tooth is then put back into its socket; permitted to become firm, and the cavity is then to be plugged: this I always did with full success.

It has sometimes happened that a dentist has extracted a sound tooth for a bad one, either by his neglect in ascertaining the tooth to be extracted, or by misinformation from the patient. If such tooth is replaced in its socket immediately after extraction, it will certainly become as firm and as useful as ever.

All that has been said will prove, I hope, that a tooth

taken out of its socket and put back in its place, will become firm and useful; therefore, if a tooth taken from another subject, the root of which is of the same shape, length and size, is placed in the socket of the tooth extracted, it will certainly become as solidly fixed as the original one. But the dentist who transplants it, must judge that the roots of both teeth are precisely alike in size and shape, before he sees either: that being impossible, the operation can therefore not succeed.

In the year 1801, I was requested to call on Miss B., a young lady of great respectability, who had suffered extremely from pain in a front tooth. I found it was the canine tooth of the left side of the upper jaw which caused the violent pain. I was requested to extract the tooth affected; but I observed to her that the loss of that tooth would be very great, and that it might be preserved by replacing it in its socket after extraction, and that it would become as firm and as useful as ever. After explaining the manner in which I should perform the operation, she consented to have it done, and I proceeded in the following manner. I extracted the tooth with a straight forceps, cleared the cavity of its carious parts, filled it with gold, washed it in warm water, and inserted it in its *alveola;* all this was done in the space of five minutes. I then requested Miss B., to bite a piece of flat cork (which I prepared for the purpose,) several times in the course of the day, and to wash her mouth frequently with a slight astringent liquid which I prescribed; if I recollect right, the tooth was perfectly firm the twelfth day. That tooth rendered service to the lady for nearly eighteen years, as I extracted it I believe seven or eight years since, having become more carious and therefore troublesome.

It is to be observed, that it is only the incisores and canine teeth which are attempted to be transplanted, as they have but one root. It is therefore the same species of teeth which I have extracted, plugged and replaced in their sockets; I have performed the same operation frequently in the course of the last twenty-five years of my practice, with the same success as in the case last mentioned.

I will however, detail the particulars of one case, in which the tooth extracted and replaced was a small molares of the under jaw. This operation was performed in the mouth of a lady of this city, Miss ******, and I expected complete success; but on examining the tooth after extraction, I found that the extreme end of the root being bent, it broke in the extraction, and the piece so bent, (about an eighth of an inch,) remained at the bottom of the socket, notwithstanding

the accident, I replaced the tooth in its socket after having plugged it, in the hope that a callus might be formed, and a junction take place. The tooth was replaced for nearly a month before it became firm; but no kind of inconvenience was experienced, either by inflammation or pain. The tooth lasted in that situation full six years, when it became troublesome and a little loose: I extracted the tooth in the course of the seventh year, the broken piece of the root remained at the bottom of the socket, which healed completely, and has never given trouble from that day to this, although six or seven years have elapsed.

About eight and twenty years since, I was sent for by Miss G. a young quaker lady from Virginia, who was in great pain from a large molaris of the upper jaw, and confined to her room. After examining the tooth complained of, I found it to be a very large one with but a small cavity in the centre of its crown. I proposed the operation of partially extracting the tooth, replacing it, and to plug it when it had become firm. The late Dr. Kuhn attended the young lady at the time. I informed him of my intention, and explained to him how I should proceed; the doctor having advised the young lady to let me make the experiment, the operation was performed. About two or three weeks after, I filled the cavity with gold, and I never heard any more complaint from it.—I might cite several other cases of the same kind, which succeeded as well; but let these suffice.

My opinion therefore is, that teeth cannot be transplanted from one mouth into another, so as to answer the intended effect; that is, that the transplanted tooth will not become as firm and as useful as the one which it has replaced, and last as long till destroyed by caries or accident. The reasons which I give in support of this opinion are those already advanced, that the root of the tooth which is to replace the defective one, should be of the same *length, size and shape* of the root of the one which is to be replaced, and that the dentist is obliged to judge of that without seeing either. I therefore believe that there are a thousand chances to one, against the success of the operation of transplanting teeth from one mouth into another, if not entirely impracticable.

ART. IV. *Case of Strangulated Hernia.* By THOMAS T. HEWSON.

BEING under obligations to Dr. Webster for the share I had in the treatment of the following case of strangulated umbilical hernia, I take a pleasure in complying with his wishes, by presenting it for publication in the Recorder.

Mrs. S., aged 70 years, of an excellent constitution, had been subject to a rupture at the navel for twenty years. On the 15th of June she took a great deal of exercise, ate a hearty dinner, and, immediately as she rose from the table, she was attacked with violent colicy pains accompanied by vomiting. In the evening, I was desired to visit her in consultation with Drs. Hartshorne and Webster; cathartics, the taxis, &c. were ineffectually employed. From the history of the case, the only chance of safety for the patient seemed to be in the operation, which was performed at noon on the succeeding day.

I first made a crucial incision embracing the whole extent of the tumour, and dissected back the integuments. This brought the hernial sac into view. A small opening was then made in the apex so as to admit the finger, and then dilated as far as the adhesions of the contents would permit. I proceeded to remove these adhesions and found a large volume of thickened omentum covering a portion of perfectly healthy colon, on the pubic side of the tumour, concealed by the mass of omentum and colon there was a loop of small intestine of a dark red colour. Introducing the tip of the finger as a director, the crooked bistoury was passed under the pubic margin of the umbilical opening and carried in the direction of the linea alba, so as to remove the stricture. The strangulated portion was then reduced with ease. I next attempted to reduce the colon and omentum; for this purpose a free division of the latter was cautiously made; but although the contents of the colon could be freely moved, the gut could not be returned into the abdomen. Under these circumstances as the protruded parts were not in a state of strangulation, it was deemed most advisable, after securing the bleeding vessels, to lay down the omentum on the colon, and bring the integuments together so as to close the wound, compresses and a broad bandage were applied to retain the parts in this position.

In the evening she was calm and comfortable. On the 17th after the administration of an injection the patient had very copious alvine discharges. As she was troubled with frequent cough she was directed to take small doses of the tinc-

ture of opium with mucilage. On the 18th she stated she had passed a comfortable night. But she complained of flatulence and distention of the abdomen. Bowels not moved since yester evening. She was directed to take rhubarb and magnesia, of each 10 grains, every two hours until free evacuations were procured. In the evening she had taken three doses without effect. Pulse weak and fluttering, tongue furred. We observed a dark brown discoloration of the skin extending below the umbilicus. On the 19th her bowels had been freely moved, pulse good, and she was relieved of the flatulence and distention she had complained of yesterday.

On the 21st there were considerable fetor and a copious purulent discharge. At the angle between the flaps towards the sternum, and at the angle on the left side, and at the angle towards the pubes there were portions of omentum in a state of mortification. The wound was dressed with dry lint, over which was applied a pledget spread with tar ointment with a view of correcting the fetor. No change took place from this period until the 29th, when the sloughs were removed: the integuments surrounding the base of the tumour were much excoriated. To relieve which, the parts were dusted with powdered carbonate of zinc.

On the 1st of July cicatrization had commenced and the excoriations were healed. On the 12th there was a sinus under one of the flaps, from whence a large slough had been removed; on the 29th ultimo, in order to make gentle pressure on the part strips of adhesive plaster were applied.

On the 14th, two of the ligatures were removed, and on the 11th of August, the remaining ligature came away, and the wound was cicatrized, excepting the fistulous opening where this ligature had been lodged. In a few days after this, the cicatrization was completed.

The surgical writers who have treated of umbilical rupture, acknowledge the general want of success that has attended the operation, when symptoms of strangulation were present. Mr. Pott expresses his belief, that the bad symptoms observed in exomphalos, are most frequently owing to disorder of the intestinal canal, and are not the result of mere strangulation. Hence he thinks the necessity for the operation is less urgent, and its success less frequent, than in the other forms of hernia. And Mr. Lawrence states, that the cases which have come under his notice, induce him to concur in this opinion, respecting the great fatality of the operation for strangulated umbilical rupture.

The operation, as more generally performed, consists in

the free division of the hernial sac, so as to bring the whole of its contents into view. Some late writers, as Sir A. Cooper and Mr. Lawrence, in order to diminish the risk from inflammation and irritation, supposed to attend all wounds of the peritoneum, propose the division of the tendon, without implicating the hernial sac. Though sensible of the deference due to the opinion of such experienced surgeons as Cooper and Lawrence, I think myself justified in having preferred the more ancient practice of laying open the hernial sac. In such a case as the one here recorded, where the strangulated portion was involved in a great mass of omentum and colon, the former firmly adhering to the sac, if the complete exposition of the contents of the rupture were not made, the relief would be precarious, if not impossible. In recommendation of the operation proposed by Sir A. Cooper, Mr. Lawrence refers to two cases successfully treated by the former distinguished surgeon.

The profession may not, however, view with indifference the case I have recorded, where the division of the sac was crowned with success, when so many failures have occurred. Future experience may decide on the preference to which either mode of operating is entitled, or point out the circumstances to which each is particularly adapted. It is by the faithful communication of what we have observed, that the principles of sound practice can be established, to attain which the observations of an individual are generally too limited.

In reviewing the various steps of the operation, it may be justly deemed, that the efforts to free the omentum and colon from the adhesions they had contracted, and to restore them within the cavity of the abdomen, were too pertinaciously continued. Perhaps when the strangulated intestine was liberated and reduced, it would have been more strictly in accord with the true indications, on such occasions to have refrained from all further attempts at reduction. The parts which had been long protruded, had probably acquired a condition whose well being was endangered, by an officious zeal to restore them to their original position. How often does it happen, that the surgeon fails of success, by persevering in mechanical efforts, when the sole reliance should be in the natural powers of the system.

We select the following six papers from the Transactions of the Medico-Chirurgical Society of Edinburgh, Vol. 2. 1826. The remaining part of this highly interesting volume, embraces twenty-one articles, which we propose to analyze for our next number.

ART. V. *Account of the Exanthematous Ophthalmia, with observations on its treatment.* By JAMES WARDROP, Surgeon extraordinary to the king.

I HAVE remarked elsewhere,* that there were several distinct species of ophthalmia not accurately described by nosological writers, but which, in a practical as well as pathological point of view, it was important to distinguish from each other. Besides the rheumatic inflammation of the eye, the peculiar symptoms and treatment of which I have attempted to point out, there is another species of ophthalmia, possessing as distinct a character and requiring as peculiar a treatment. This ophthalmia has not only been superficially noticed, but has generally been confounded with scrofulous inflammation, a disease quite distinct from the exanthematous ophthalmia, the species of inflammation now to be described. The word scrofula has indeed been applied in a more vague and indistinct manner, than perhaps any other term in medicine; for a peculiarly bad constitution, a tumour the precise nature of which is not understood, or an ulcer very difficult to heal, is frequently denominated scrofulous. It must, however, be admitted that persons of a scrofulous constitution, are very subject to the exanthematous ophthalmia, from the same causes which render them also particularly liable to many other diseases; but neither the character of the ophthalmia, nor the eruptions with which it is connected, are necessarily derived from a scrofulous diathesis, nor does the disease appear in those alone where the scrofulous diathesis can be detected.

The species of ophthalmia, to which the term exanthematous is here meant strictly to apply, is so denominated, from being always either accompanied or preceded by some eruptive disease. Eruptions of the scalp, and discharges behind the ears, so frequent in children, are the affections with which this ophthalmia is most commonly connected. These diseases alternate with the disease of the eyes, the latter becoming

* See Trans. of the Medical and Chirurgical Society.

affected, when the eruption or discharge disappears; whilst, when either of *these* returns, the eyes recover. This ophthalmia also sometimes succeeds measles, scarlet fever, and other exanthematous diseases, but usually appears a considerable time after these affections have subsided, and when some eruption about the head seems more immediately connected with its appearance.

The symptoms of the exanthematous ophthalmia are very characteristic; for, besides being connected with eruptions, and confined to young people, the excessive intolerance of light, the enormous secretion of tears, and the relief from forcibly squeezing the eyes, are symptoms quite peculiar. The patient afflicted with this disease, can scarcely hold up his head, and if he is desired to open his eyes, he is affected exactly as if he were looking on a mirror reflecting a bright sunshine, every attempt causing a profuse gush of tears, and being instantly succeeded by a violent and involuntary squeezing of the eye-lids, and knitting of the eye-brows. He excludes all light, not only by holding down his head and squeezing the eye-lids together, but by pressing a handkerchief firmly on them, or by resting his face against a chair in some dark corner of the room. When in bed he lies with his face buried in the pillow, a circumstance, which of itself points out the peculiarity of this inflammation, and distinguishes it from all others.

The intolerance of light is always most severe in the morning; but in the afternoon it sometimes remits so much, as to allow the patient to open his eyes, and see to a very considerable degree, for some hours. The tears, besides being of an extraordinary quantity, are of an acrid, irritating quality; producing violent paroxysms of sneezing, scalding the cheeks, the alæ of the nose, and the lips; so that these become inflamed and swelled, and sometimes covered with pustules and cutaneous ulcerations. The eye-lids are also swelled, and have turgid veins on their surface. On trying to force them open, a torrent of tears gushes out, and it is not without occasioning great pain, that a small portion of the globe can be exposed. An attempt to get a view of the cornea gives great pain, and it is almost impossible to succeed. The palpebræ, as well as the sclerotic conjunctiva, are but slightly reddened; the vessels appearing as a few distinct trunks, instead of the diffused redness observed in many other inflammations. In general, both eyes are attacked with this disease, though one more violently than the other.

Along with these local symptoms, there is always more or

less constitutional excitement. The pulse is frequent and agitated, the tongue is particularly white, the primæ viæ greatly disordered, the abdomen tumid, the skin sallow, and there is great loss of flesh.

The disease sometimes remains in its aggravated form for many days or weeks; at other times it remits, and the eyes can be opened for a few days; after which it recurs as violent as ever. The constitutional symptoms likewise vary in severity at different times, and in this manner I have often known cases which had continued for many months, and even years.

When the eruption on the scalp recurs, or the discharge behind the ears increases, then the inflammation of the eyes generally subsides, and the cornea can be examined. Besides a greater or less degree of fulness of the vessels of the sclerotic and palpebral conjunctiva, there will generally be perceived one or more specks on the cornea, and even sometimes a distinct ulcer. These, however, are trifling, and indeed there is nothing more remarkable in the history of this disease, than the slight injury which the organ sustains, after being long and severely affected. No puriform discharge, or morbid secretion from the meibomian glands, accompanies this inflammation; nor have I ever known the structure of any part, except the cornea, permanently changed. This fact, along with the other characteristics of this ophthalmia, particularly its metastases with eruptions of the adjacent skin, makes it probable, that in this disease the inflammation is seated in the conjunctiva.

Treatment.—The peculiarity of this ophthalmia is also marked by the effects of remedies in the treatment of the disease. General or local depletion, the application of stimulating ointments, of opiates, astringent collyria, &c. which relieve some of the other inflammations of the eye, have little or no influence in subduing the exanthematous ophthalmia. Hence the numerous cases where this disease, notwithstanding the use of a variety of local remedies, has continued for many years. It is, however, a disease as much under the control of medical treatment as most others. The general plan of management rests but little on local applications. The chief attention, more particularly in the beginning of the treatment, is to be directed to the constitutional symptoms, and no local application, except cleansing the eyes with warm water, ought to be employed; for if stimulating ointments or similar applications be used, even while the constitutional symptoms are yielding, they will rather prevent any amendment taking

place in the local inflammation, and thus baffle the surgeon, while, by leaving the eyes untouched, their amendment will serve as a guide for conducting the general treatment; and it will invariably be observed, that, as the constitutional ailments abate, so will the local symptoms yield, and in many cases the local disease will even be completely removed by the general treatment alone. Local remedies are not, however, to be entirely disregarded, for, in many instances, a painful state of the eyes continues, notwithstanding the most judicious general management; which yields with great rapidity, when local remedies are properly employed. Indeed, the observation now made, applies to the treatment of all diseases; for in every local disease, constitutional symptoms accompany, which must first be combated; and when they are removed, the local treatment afterwards required, is usually very simple.

The general treatment which is commonly necessary for the cure of the exanthematous ophthalmia, consists of first completely evacuating the bowels, and afterwards regulating them; of giving alterative and tonic remedies; and of producing an artificial discharge. Even when this ophthalmia appears in a feeble and emaciated child, it will usually be found, that by the exhibition of purgatives, feculent matter, both unnatural in quantity, and of a bad quality, will be evacuated; and, until its evacuation has been effected, other remedies avail little. One grain of calomel with three of rhubarb, given at bed-time, and repeated every other night, four or five times, whilst jalap or senna is taken the alternate mornings, will generally answer the purpose of bringing away the feculent contents of the primæ viæ. But whenever the quality of the evacuations improves, these medicines must be given with caution; and one dose of the rhubarb with calomel, given only once in six or eight days, and the senna or jalap occasionally, will be sufficient. For though the greatest benefit will be obtained by evacuating the bowels, violent purging will be found equally prejudicial. When the treatment has been so far advanced, that only one dose of calomel appears necessary in six or eight days, then at this time tonic and stomachic medicines may be advantageously administered. Of these I have found none so generally useful as the carbonates of soda or potass, either given singly, or combined with rhubarb and the bitter infusions. In some instances the mineral acids have been very useful, and also the preparations of iron. Whilst using either of these remedies, much attention is also due to food and habits of life. All

wines and malt liquors are particularly hurtful, and the patient should live chiefly on farinaceous vegetables, with but a very small proportion of animal food. The body should not be loaded with clothes, and the head particularly should be slightly covered; protecting the eyes with only a single and narrow fold of black silk, hanging loosely over them, and not wearing a large bonnet. The hair ought to be cut very short, and the greatest advantage will be found from sponging the head and neck with water every morning; using it at first of an agreeable temperature, and making it colder by degrees, particular care being taken to dry the head well afterwards.

The good effects of this system of general treatment will soon become manifest, and other means need not be too hastily resorted to, whilst there is a decided progression in the state of the eyes. However, in some cases of the disease, and when the local symptoms cease to make a daily improvement, additional means become necessary. Perhaps the next measure which ought to be employed, is the application of a succession of small blisters behind the ears; or when the skin is unusually tender and prone to disease in that part, it is preferable to put a large one on the nape of the neck. Should there be, from any cause, an objection to the use of blisters, equal advantage may perhaps be derived, from the ointment composed of the tartrite of antimony, applied so as to produce a plentiful crop of pustules on the scalp. I am in the habit of employing a succession of blisters, in place of keeping one constantly open, but each mode may have its advantages, and one may occasionally be preferable to the other.

In this stage of the treatment, the intolerance of light and excessive lachrymation become subdued, so that the patient is now enabled to open his eyes, particularly in a moderate light. Frequently, however, the eyelids still remain tumefied, and the vessels of the palpebral conjunctiva, though not much increased, continue enlarged, and distended with blood. If a superficial incision, or rather a very slight scratch be made, with a sharp edged instrument, or common wedge-shaped scarificator across these vessels, the under eye-lid being previously completely everted and pressed on the edge of the orbit, an astonishing quantity of blood will sometimes flow, and the relief obtained from this simple operation, is often very remarkable. I have frequently observed, that the quantity of blood which comes away by scarification, in different forms of ophthalmia, is never in proportion to the redness of the palpebral conjunctiva; and in the exanthematous ophthalmia, the redness is

always less in proportion to the flow of blood than in any other inflammation. This operation is followed, in many instances, by the most marked alleviation of the symptoms, in which case it may be a few times repeated, until the increased fulness of the vessels, and thickening of the eye-lids are removed, and one or two days allowed to intervene between each operation. Sometimes, however, the mere abstraction of blood is not sufficient to produce such a beneficial result, and then a small piece of the common ointment of the red oxide of mercury, may be put within the eye-lids, immediately after the blood has been withdrawn. This application sometimes excites but little, and at other times a good deal of irritation, but it is almost invariably followed by a decided diminution of the symptoms. When these have so far subsided as to admit of an examination of the cornea, one or more specks will be generally discovered, and in many instances a distinct ulceration; in either of which cases, the stimulating ointment is particularly useful, and ought to be re-applied at intervals of four or eight days, until the transparency of the cornea be completely restored.

When the disease has been severe, and of long duration, though the system of treatment now recommended be followed with apparent success, all the symptoms being subdued, yet care and attention are still requisite: for it often happens, that the patient's general health gradually declines; the digestive organs become deranged; and when this takes place, a fresh attack either of the ophthalmia or eruption supervenes. To prevent such a relapse, as well as to ward off attacks from other organs, the seton and pea issue are particularly calculated. I have therefore laid it down as a general rule, to adopt one or other of these useful remedies, as shall appear most fit; at the same time, for a long period, to pay attention to the state of the primæ viæ, and to have recourse to country air or sea-bathing, along with such other means as are generally employed to invigorate the constitution.

ART. VI. *Case of Extraction of Calculus from the Female Bladder, by Dilatation of the Urethra.* By ROBERT HAMILTON, M.D. Secretary.

IN the last volume of the transactions of the Medical and Chirurgical Society of London the following remark is made by Sir Astley Cooper, in a paper on the dilatability of the female urethra: "Further experience will be required to determine if it will be best to dilate the meatus in a few minutes, or hours, or in several days, by more gradual dilatation." I take leave to present the following case, as it bears directly on this point; and is one of a class which, from its nature, will not accumulate very rapidly on our hands.

Since the attention of the profession has, within these few years, been directed to this subject, the facts brought into notice would go to show, that the dilatation of the urethra is a very easy matter. Cases are on record, in which calculi, of great magnitude, obtained a spontaneous egress. With the assistance of sponge-tents, dilatation has generally been accomplished in a few days; and since more powerful instruments have been invented, it has been effected in as many hours: Nay, in some cases, it has been represented, that a common probe or catheter, is, with the finger, all that is necessary for succeeding in our object in a few minutes. It was not so simple a matter in the case I now present.

In November, 1823, Mrs. F., æt. 55, a stout hale woman from Alloa, presented herself to the New Town Dispensary, labouring under symptoms of stone. The complaint had existed for six or eight years. Having, by sounding, satisfied myself of the existence of calculus, I informed her, that the only relief which could be afforded was by extraction; which would probably be effected by dilatation, without any cutting. She, that evening, took some opening medicine; and next day at 3 P. M., I introduced Mr. Weiss's dilator of the female urethra, and turned the screw up two teeth; so dilating the blades to the extent of one fourth of an inch. I returned in an hour, and found my patient complaining much of the pain that was produced. A few drops of blood were oozing from the meatus. I turned the screw another point, with great increase of suffering. An opiate was administered. In an hour and a half I again returned; found the oozing of blood more considerable, and the pain more intolerable. It required much persuasion before I was permitted again to turn the screw, and separate the bladder to the 4th division

marked on the scale. At two several intervals of an hour and a half, I raised it two other points in the scale, and thus brought the dilatation to equal three-fourths of an inch. This, it will be observed, was effected with much severe suffering, in six hours.

At 11 P. M. I withdrew the dilator, and could, of course, easily introduce the forceps. With a pair of strong polypus forceps I could grasp the stone, but found the hold I maintained was not sufficient. The stone slipt in my endeavours to extract. With a lithotomy forceps, of a small size, I found that the blades were so large and thick, that I could obtain no firm grasp; they only rasped along its partially embraced surface.

After satisfying myself that the dilatation was insufficient, I again introduced the instrument, and raised the screw to the 7th point in the scale. A full opiate was administered, and a respite held out, for sleep, during the night.

At 7 next morning it was found she had suffered much during a sleepless night. The parts were exquisitely tender, and bleeding a little. The screw was raised another point; and again another, to the 9th, in an hour afterwards. This brought the dilatation to 1 1-8th of an inch.

I was now convinced that the patience of my patient was completely exhausted. Often had she expressed a wish that the process had never been commenced; and a strong conviction that it was more painful than any cutting. Being therefore fully persuaded that she would no longer submit to the further use of the instrument, and solicitous that all her suffering should not be useless, I provided myself with a probe-pointed bistoury, in case the dilatation was not even now sufficient; and at 9 A. M. determined, with the assistance of my friend Dr. Molison, to do all in my power for her relief.

A firm hold with the lithotomy forceps could now be procured. The position of the stone could be adjusted in the most favourable manner; and all the force that was regarded safe could be steadily applied. This was done for four or five minutes; but the urethra would not yield. After the dilatation and the force had been carried to what I conceived the extreme limit, I introduced the bistoury, and cut at the angle formed by the junction of the bladder and urethra, in a direction upwards and outwards. The bridle, which had obstructed the progress of the stone, was thus divided, and it was now easily extracted.

The incision must have been to the most trifling extent, as

there was no hemorrhage; and the patient was never sensible of any incontinence of urine. Another opiate was administered. The patient lay in bed for the most of the day, and made water at regular intervals. She had no unpleasant symptom whatever after the operation; next day was walking about the house, and in a few days returned home in perfect health. She came to town a few months ago, and called to inform me she was completely cured.

The stone weighed precisely one ounce. Its long circumference measures 4½ inches. Its short 1¼ inch.

Thus was dilatation of the female urethra effected, in this case, to the extent of one inch and a quarter, in eighteen hours of severe suffering. The extent of suffering was shown by the hemorrhage, and the exquisite sensibility of the parts, as well as by the unceasing complaints of a woman of a naturally strong mind; and this state of things very naturally suggested the inexpediency and hazard of delay. The pain was of that character, that, had it been continued much longer, inflammation would have been the consequence, and this would have spread to the surrounding parts.

The conclusion which I feel inclined to draw from this case is, that, when the stone is of considerable size, it will not, in all cases, be possible, with the utmost care, to effect the necessary dilatation in a short space of time; and that the more gradual dilatation, carried on for several days, by means of the sponge-tent, will prove safer and easier than the more rapid operation of the new invented dilator.

ART. VII. *Cases illustrating the contagious nature of Erysipelas, and its connexion with a severe affection of the throat.* By JOHN STEVENSON, M. D. Arbroath.

In a letter to Dr. THOMSON, late Regius Professor of Military Surgery.

MY DEAR SIR—I enclose a short abstract of some of the cases which, in our late conversation, I mentioned to have occurred in my practice here, tending to prove the contagious nature of erysipelas, and to show its connexion with a peculiar and severe affection of the throat, of which I do not recollect to have met with any description. This affection of the throat occurred so frequently in persons who had been much with erysipelatous patients, that I could not doubt their identity; and I came finally to the conclusion, that it was in reality

erysipelas of the fauces, spreading occasionally to the adjacent parts in different directions. The febrile symptoms by which it was ushered in, were generally severe, even in the milder cases; very full and frequent pulse; severe pain of the head and back; restlessness, and great heat of the surface. The period at which the affection of the throat came on, after the accession of the fever, varied from the second to the sixth day. It commonly began with a red or purplish blush, more or less extensive, over the velum pendulum and uvula, accompanied with very little tumefaction, but with considerable pain in swallowing: often, after a few days, excoriation of the inflamed surface followed, with superficial ulceration, which at times soon healed, but at other times spread and discharged a good deal of purulent matter. In many cases the disease terminated without extending farther than the parts mentioned, but in a few it spread to the larynx, producing a state of respiration very like that of idiopathic croup; in others it extended to the pharynx and œsophagus. When the last became affected, fluids, and even solids, could be partially swallowed without much apparent difficulty; but after a few seconds, pain was felt in the course of the gullet, an inverted action began, and they were wholly or partially returned to the mouth. In some protracted cases, glandular swellings appeared in the neck, which suppurated externally.

This disease was readily distinguishable from cynanche tonsillaris, by the want of swelling, by the redness being more diffused, and by the pyrexia being generally greater than could have been expected from the degree of local affection. From croup it was distinguishable by the larynx being affected in a small proportion only of the cases, by the inflammation not commencing there, at least in any case which came under my observation, and by the age of the patients. From scarlatina, it was distinguished by the absence of cutaneous eruption, and by its attacking persons who had already had that disease.

Copious and repeated bleeding, with brisk purgatives, and in every case where the throat was severely affected, the application of a large number of leeches to the neck, appeared to me to be the mode of treatment which was most successful. All the cases that came under my management, both of the common erysipelas, and of the affection of the throat, were treated in this manner. I have stated the event of all those detailed, and it affords a fair specimen of the general results of the cases which came under my care.

I have been anxious to condense this communication as

much as possible. I have selected such cases only as appeared to illustrate the connexion between the two disorders in the most remarkable manner; though a great many more occurred in my practice, tending to support the same conclusion, but not perhaps so strikingly. Of those cases which occurred among my relations, you will observe that Mrs. S. *jun.*, was seized precisely at the time I was most engaged in attending patients labouring under this disease. In her case, as in many others, external erysipelas supervened to the sore throat, but I saw no instance of the reverse. In several cases, when ulceration did not take place, the redness speedily disappeared, seeming, as it spread to the contiguous parts, to leave those originally affected. This was remarkably exemplified in J. H. (No. 2.) No redness could be perceived on looking into his throat after the second or third day, though the disease lasted several weeks. No person, in seeing M. M., (No. 3,) or R. P., (No. 8,) could have given the complaint any other name than cynanche laryngea, and yet, taking all the circumstances into consideration, scarcely a doubt can exist of the inflammation in these cases being of the same kind with that in the others, only affecting parts, the structure and functions of which were different. I am, &c.

JOHN STEVENSON, M.D.

Case 1. Mrs. H., Oct. 26, 1821.—Erysipelas of the face and head; severe case; high fever and delirium; no affection of the throat.—*Recovered.*

2. J. H., son to No. 1, November 7, 1821.—Throat affected in the manner described; tedious case; inflammation spread successively to the pharynx and œsophagus; slight erysipelas of the face came on during the second week, but soon subsided; but the affection of the throat continued a considerable time afterwards.—*Recovered.*

3. M. M. ætat. 50, attended on No. 1, November 23, 1821.—Severe affection of the throat, which soon spread to the larynx; the danger appeared imminent; the respiration resembling that of severe idiopathic croup; no external erysipelas.—*Recovered.*

4. H. H., daughter to No. 1, November 25.—Considsrable pyrexia; slight affection of the throat, which did not spread nor ulcerate; no external erysipelas; soon got well.

5. J. S., January 18, 1822.—Throat affected severely, very high fever; had visited several times a neighbour, who died of erysipelas of face and head; larynx a little affected;

erysipelas of the face of a mild description, came on about the 8th day, when the other complaint was declining.—*Recovered.*

6. Mrs. N. frequently visited No. 5, January 25, 1822.—Severe and tedious case of erysipelas of face and head, with a high fever and delirium; throat not affected.—*Recovered.*

7. Mrs. H. February 5, 1822, frequently visited No. 6.—Affection of the throat as described; tedious case, but not severe; lasted five or six weeks; no external erysipelas.—*Recovered.*

8. R. F., ætat. 60, frequently visited No. 5, who wrought in the same tanyard with him, February 5, 1822.—Affection of the throat as above, appeared slight at first, but spread to the larynx on the third day, and he died on the fifth, with all the appearances of a child in severe croup.

9. —— S., attended on No. 7, February 15.—Severe affection of the throat, but of short duration; no external erysipelas.—*Recovered.*

As soon as this patient was taken ill, she went home to her parents, who resided at some distance. I afterwards learnt that they were very soon seized successively with similar complaints, and that the mother died a few days after being attacked.

10. M. S. attended on No. 7, after S.'s seizure, February 25, 1822.—Severe erysipelas of one arm; high fever and delirium; no affection of the throat.—*Recovered.*

11. Mrs. T., August 12, 1822.—Severe erysipelas of arm and shoulder, terminating in extensive suppuration of the whole cellular membrane, and death.

12. P. K., son-in-law to No. 11, August 20, 1822.—Severe affection of the throat, chiefly of the pharynx and œsophagus.—*Recovered.*

13. Mrs. R., sister to No. 11, September 17, 1822.—Affection of the throat, chiefly of the pharynx; tedious but not severe.—*Recovered.*

14. J. R., son to No. 13, November 1, 1822.—Severe case of erysipelas of face and head: very high fever and delirium; no sore throat.—*Recovered.*

15. December 1, 1822.—I was called to W. R., father of the last patient, and found him affected with a sore throat, exactly similar to that of the others. He had been seized with a rigor a few days before, and the throat was felt painful next day. The febrile symptoms less severe than in the generality of cases.

The following cases occurred among my own relations.

16. Mrs. S. *jun.*, February 6, 1822.—Rigor; intense pain of head and loins; very high fever. Second day, severe sore throat, with total inability to swallow. Seventh day, erysipelas of the face gradually spreading over the whole head; considerable purulent discharge from throat. Fifteenth day, critical sweat.—*Recovered.*

This was the severest case I had an opportunity to see, of the external erysipelas supervening to the sore throat. The erysipelas in most other cases was mild, and not attended with much burning pain; generally commencing when the fever began to subside.

17. M. S., attended on No. 16, during the first week of her illness, February 14, 1822.—Rigor; high fever; delirium and stupor from the commencement; erysipelas of face, head, neck, and shoulders, extending down over part of the trunk; no sore throat. Died on the 13th day.

18. C. S., much with both Nos. 16 and 17, March 2, 1822. —No rigor; severe pain of loins; no headache; very high fever for a week; pulse after that period continued about 105 or 110 beats in the minute, till the disease terminated; sore throat as described, began on the fifth day, ulcerated, and discharged pus for several weeks; a blister, applied to the neck about the seventeenth day, produced erysipelous inflammation over the whole thorax, but healed readily; disease lasted near nine weeks.—*Recovered.*

19. Mrs. S. *sen.*, constantly with the above, April 3, 1822. —Severe case of erysipelas of face and head; high fever, delirium, and stupor; no sore throat; crisis on the 10th day.— *Recovered.*

20. Mrs. S., much with all the above, April 10, 1822.— Very high fever; severe sore throat; larynx became affected on the fifth day; no external erysipelas. The disease in this case appeared to be cut short by large bleeding. After two copious venesections, 20 leeches were applied round the neck, on the appearance of the laryngeal symptoms, and bled so long and violently as to excite considerable alarm. She was much weakened, but the disease subsided immediately afterwards.

21. M. G., much with Nos. 2 and 3, April 10, 1822.— High fever for a few days, with slight affection of the throat. —*Recovered.*

N. B.—When this paper was read, (July 7, 1824,) several members of the society mentioned having observed similar

facts within the last few years, although the succession of cases, where communication with persons already affected had taken place, had not been traced to so great an extent. Several members had seen the affection of the throat here described, supervene on the erysipelas, even in its later stages. In three cases, mentioned by Dr. Abercrombie, Dr. Hay, and Mr. Bryce, the inflammation appeared to have spread from the fauces to the external surface, by the membrane lining the internal nares, the part of the skin first affected having been, in the first two cases, at the orifice of the nostrils, and in the last at the orifice of one of the lachrymal ducts.

ART. VIII. *Case of Empyema, successfully treated by Paracentesis Thoracis.* By JAMES PITCAIRN, M. D., Fellow of the Royal College of Surgeons.

SUSAN SMITH, ætat. 11½, had been an uncommonly healthy child, and of a robust stout make. She got a fall about a week ago, on her left side, by which it was reported she had hurt some of her ribs. During the night of the 19th, she complained of the pain being much more acute, and of its having extended over a large portion of that side; these symptoms were accompanied with cough, quick breathing, and general feverishness.

I saw her on the 20th at 10 A. M., at which time she laboured under violent difficulty of breathing, amounting to orthopnœa,—great pain of the left side,—constant tickling cough,—hard, full and quick pulse, and flushed face. The pain of the side was greatly aggravated by the cough, or by an attempt at deep inspiration; she was largely bled, (viz. to ℥xiv.) from which she experienced instant relief; she was ordered a brisk purgative with antimonials. On examination of the side, none of the ribs were found injured.

3 P. M.—I was told the relief from the bleeding was but of short duration; the pain and difficulty of breathing being now as bad as ever. The blood drawn was extremely buffy; the purgative had operated twice. She was again bled to ℥xij, which produced syncope; it was, however, followed with great ease.

[illegible] M.—The pain was still very acute, and the pulse sl[illegible]although the breathing was considerably better. She wa[illegible]ain bled to ℥vj, from the arm, and twelve leeches were

applied to the side. The blood was very buffy; the antimonials and purgatives were continued.

It would be tedious and useless to go through the details of the case at this time: suffice it to say, that, notwithstanding the repeated application of leeches, blisters, tartar emetic ointment, with the internal exhibition of digitalis, squills, hyosciamus, hydrocyanic acid, and preparations of iron and other tonics, there always remained a sense of deep-seated pain in the thorax, accompanied with cough and short breathing, which were readily aggravated by any injudicious exposure to cold air. The pulse kept uniformly above 110, and ranged from that to 130.

The only effect, therefore, of the active treatment in the first stage, was to subdue the violence of the acute symptoms.

At the end of the fifth week from the first attack, she became very much emaciated—had constant cough, with copious purulent expectoration,—frequent attacks of diarrhœa, and occasional hectic paroxysms.

After a pretty long course of various tonic remedies, her parents were advised to try the effect of sending her to the country, which was done about April, 1823. From this she returned without the least benefit in a short time, nor could she be prevailed on to prolong her stay.

Although she had been in town for a long time, I was not sent for until the 13th September, when I was requested to visit her, on account of the extreme urgency of the cough, and the very profuse expectoration. The difficulty of breathing also, was such as completely to prevent her lying down; in fact, the only relief she had, was in sitting on a low seat, with her body bent forwards on her knees. At this time the purulent expectoration was enormous,—the cough was literally incessant,—the countenance anxious, and the lips livid.

Observing something peculiar about her shape, I ordered her to be stripped. There was the most marked difference in the relative capacity of each side of the thorax; the left being very much enlarged and prominent in all directions. The dorsal vertebræ were forced to the right side; the base of the heart was found pulsating under the *right* nipple, and its apex about two and a half inches more to the *right;* its action was regular, but very rapid, being 145 in the minute; the respiration was short, rapid, and difficult.

I spoke to the parents of an operation as the most likely means of doing her any good, which they were decidedly against, as well as the patient herself. Dr. Thomson saw

her with me on the 15th, and strenuously advised the operation, as the only rational means of prolonging her life, to which the parents consented on the morning of the 16th, on the afternoon of which day I punctured the thorax. The operation consisted in making a small incision between the fifth and sixth ribs, nearly at their middle, and then passing a flat trochar and canula into the thorax.

There was evacuated at this time ℥xviij of thick pus. The immediate effect of the operation was to relieve the breathing,—greatly to mitigate the cough,—to diminish the size of the left side of the chest,—and to allow the heart to come back about an inch and a half from its former situation.

On the 17th and 18th, there was discharged through the canula (which was left in the thorax) ℔.iiiss. of thick pus. The cough continued but in a very slight degree; the pulse fell to 120, and had rather increased in strength.

During the 19th, 20th, 21st, 22d, 23d, and 24th, there had been very little discharge; and as the breathing had been getting a little more oppressed, it was deemed advisable to get in a larger canula, which was done on the 25th, by which means, about ℔.iiiss. of pus were discharged through the night of the 26th.

During the 27th, there were ℥xviij; pulse 112; has no cough; bowels regular; sleeps well, and has no sweating.

On the 28th she was much the same; discharge amounts to ℥xiv. Dr. Cullen visited her, and he makes the following stethoscopic report. "The whole of the left side of the thorax was greatly enlarged; the intercostal spaces prominent; and the angles of the ribs of greater dimensions than on the opposite side. Percussion wherever practised, occasioned an uniform dull sound; there was, in fact, a total want of sonorosity. The heart was found pulsating on the right side. The stethoscope could not detect respiration under the clavicle, nor in the præcordial nor lateral regions; and it was heard in the back, only for the space of about an inch and a half, close to, and all along the vertebral column. Respiration was heard but very feebly in the highest part, also of the lung, viz. in the supra-clavicular fossa. From these symptoms it was evident, that the lung, pressed by the large quantity of superincumbent fluid, was adhering close to the spine, and to the upper part of the thoracic cavity, and consequently its tissue must have been greatly condensed."

October 1st, 2d and 3d.—She continues in the same state; pulse 114; was ordered the diluted sulphuric acid; she appears to be gaining strength.

The 4th, 5th, 6th, and 7th.—The pulse had got up to 130, without any accession of difficulty of breathing, or disposition to cough. The discharge has not amounted to more than ℥j. during these five days; she remained the same on the 8th, 9th, 10th, 11th, and 12th, without any increase of discharge.

On the 13th and 14th, the pus from the thorax amounted to ℔.iiss.; pulse 132; has no cough. At this time she had a pretty severe attack of diarrhœa, which lasted for four or five days, but was got completely the better of, by the ordinary astringents.

From this date to the beginning of November, she continued in the same state. The discharge from thorax, was daily to the extent of ℥viij, and flowed in this quantity very regularly. The heart seemed to be fixed permanently, by adhesions under the cartilages of the ribs of the right side.

From a severe indisposition of my own, I was prevented visiting her from the 7th or 8th of November, for about three months. I learnt from her parents, however, that during that period, her symptoms remained the same. The pus evacuated was pretty regular in its quantity, during the twenty-four hours, (viz. about ℥vj,) until in the commencement of January, 1824, from the canula being out, the wound gradually closed; and as soon as it was entirely shut, a small pimple appeared, about three inches nearer to the sternum than the operation wound, but between the same ribs. This grew larger and larger, till it attained the size of a small orange, when it burst, and discharged about ℔.ij. of pus. From the thorax being closed at this time, for a fort-night, the cough, difficulty of breathing, and increased size of the left side of the chest returned. But these were immediately relieved by the second opening which nature had made.

The discharge after this resumed its usual quantity (℥vj,) when very suddenly, in the beginning of May, it diminished in a very great degree, and that very irregular in its flow. From a foolish exposure to cold and wet, she got back her cough and difficulty of breathing to a considerable extent, without however, any increase of the purulent discharge.

It was deemed advisable to enlarge the opening into the thorax, which was done on the 17th May, by introducing a curved bistoury into the orifice, and cutting to the extent of three-fourths of an inch. This opening was further dilated by the insertion of sponge-tent. On taking out the tent on the 19th, there issued about ℔.iss. of pus.

The symptoms after the adoption of this plan were more and more favourable: the pus got free exit: the cough

ceased entirely; the short breathing was much relieved; the appetite became pretty good; and the tendency to perspiration was checked.

From the beginning of June until the 18th, the discharge had been gradually lessening in its quantity, and was very irregular in its flow; during some days it amounted to nearly ℥vj, while in others there was either none at all, or it appeared in very small quantity. On the 18th June, the discharge suddenly changed its appearance from being purulent, to a yellowish serum, and this continued to flow in the same diminished proportion, which the pus had done from the commencement of June.

This change in the quantity of the discharge was attended with very marked advantage to the general health; her pale sickly look in a great degree left her, and she rapidly gained flesh and strength. She was sent to the country on the 28th, at which time the amount of the serous discharge does not exceed a table spoonful in forty-eight hours.

In July the wound had been gradually closing, and the discharge getting less in quantity than when she left town; and about the 15th it was quite healed, and continued so for a fortnight, when she incautiously went to sea-bathing three or four times, by which she caught cold. The wound again opened and discharged serous fluid for ten days more, when it ultimately closed. She was at this time able at all times to romp about with her companions, without inducing any tendency to cough.

I saw her on the 23d October, 1824, when she returned from the country in perfect health. At the present date (February 1826), she continues in the enjoyment of good health, has no pectoral symptoms whatever, but the side of the chest, formerly enlarged, is now much contracted in comparison with the other, precisely as described and figured by Laennec, with a corresponding lateral curve of the spine.

ART. IX. *Case in which the Stomach of an Infant terminated in a Cul de Sac.* By JOHN CROOKS, Esq. Surgeon, Kilmarnock.

Communicated by JOHN GAIRDNER, M. D. Fellow of the Royal College of Surgeons.

THE subject of the following case, a female infant, was born on the morning of the 1st June, 1825. At that time it appeared well formed, plump, and healthy; but rather little,

and, by the mother's reckoning, born a month short of the usual time. About six hours after delivery, the child vomited a considerable quantity of a dark coloured matter, something resembling half digested blood or *coagulated* meconium. A short time thereafter she vomited again, and the appearance of the matter now thrown up was less equivocal; it was obviously blood. The infant continued to vomit at short intervals, and apparently without difficulty or pain, the same grumous matter, occasionally mixed with what little nourishment or medicine she had swallowed. On the evening of the 3d she suddenly expired. During the 65 hours that she lived, she would not suck the mother, but swallowed pretty readily such other liquid nourishment and medicines as were prescribed for her. Very little meconium passed from the bowels.

The body was inspected 14 hours after death. By this time the surface of the skin had become of a dark livid colour all over, and a considerable quantity of blood had escaped by the mouth and nose. On opening the abdomen, the stomach was observed very much distended, and occupying nearly the whole left side; the liver hung far down; and under, and between the two, the intestines, extremely small and contracted, were gathered together like a bunch of worms. The internal surface of the stomach was spongy and inflamed, and in several places was dark coloured, and blown up with numerous air vessels, apparently from gangrene. The pyloric extremity of the stomach terminated in a pouch or second stomach without outlet, a complete *cul de sac;* so that no direct communication could exist between this organ and the bowels. Into the fundus of this second stomach the pancreatic duct opened.

ART. X. *Case of Chronic Gastritis.* By WILLIAM BROWN, Esq. Fellow of the Royal College of Surgeons.

Mrs. C., aged 40, placed herself under my care in the end of August, 1825. About a month previously she was exposed to cold while sweating, and had complained ever since. She was troubled with a looseness, for which she had frequently taken purgatives. I prescribed some remedies, which benefited her, but still she continued unwell. Upon the 7th of September, the symptoms noted were pain in the stomach, immediately after taking food, without vomiting, or at least without constant vomiting, and pain on pressing the epigas-

trium. These symptoms continued almost unabated till her death, on the 23d of November.

Instead of burdening the society with a journal, I shall merely state, that pain, on receiving food or drink into the stomach, was the leading feature in the case, and, indeed, the only unvarying one. The pain, on pressing the epigastrium, was sometimes less severe, sometimes more acute; or, as she described it, a burning pain. In the course of her illness, there supervened pain lower down, about the umbilicus, and she could easily distinguish this from the original pain. Vomiting was only an occasional symptom, and was sometimes resorted to by her, for the purpose of relieving the stomach of its contents. The bowels were generally costive, and required the frequent use of purgatives and enemata; but at intervals diarrhœa came on, which was very distressing during the latter period of her life. Her pulse was seldom quicker than 90. She perspired a great deal, but had no other symptom of hectic fever. Her appetite continued excellent till within a few days of her death, but she ate very little; and, from the comparative ease she experienced when the stomach was empty, said she was sure she would be quite well if she could do without food. The emaciation was extreme.

A few days before her death, a small circumscribed swelling was observed, lying over the region of the pancreas, having the regular pulsation of the aorta throughout, and thus leading to the idea of aneurism, or of diseased enlargement of that viscus.

She was twice or thrice leeched and blistered, and had the antimonial ointment applied, but without any marked good effect. She twice used the blue pill, to the extent of slightly affecting her mouth, and said she was easier at both times. Bitters and chalybeates were used without any advantage. Opium gave her relief in all the stages of the complaint.

The body was examined thirty hours after death, and the appearances were very unexpected. The peritonæum, intestines, liver, spleen, pancreas and aorta, were quite healthy, and the mucous membrane of the stomach was alone diseased. This was in every part thickened and vascular; it had more of the *velvety feel*, and, at the same time, was more firmly adherent to the other coats, than is observed in the healthy organ. There was a small body, resembling in colour, shape and consistence, a spleen in miniature, lying near the proper spleen. This occurs occasionally, I belive, as an anatomical variety, but cannot be called a morbid appearance.

I have felt this to be an instructive case, and I trust it has not been uninteresting to my fellow members. It is a rule formed upon the soundest principles, that a practitioner should review his treatment in every fatal case: whatever be the result of the scrutiny, if it be patiently and candidly pursued, he will never fail to receive solid instruction. I have to regret, that, in the case just narrated, the treatment was at first directed too much under the impression, that the case was one of functional disorder merely, and that afterwards, when progressive emaciation was added to other symptoms, it was in a great measure palliative, in the belief, that it was an instance of structural degeneration, and therefore incurable. Perhaps it really was so, and the distance of time which had intervened between the attack and the application of remedies, renders this probable; but it must ever be remembered, that there are some diseases which become incurable, only from our having neglected to apply the appropriate remedy at the appropriate time.

We know that if, in peritoneal or tracheal inflammation, the remedy be withheld for a few hours, a case otherwise under the power of medicine, will become really incurable; and the best treatment, if then employed will be of no avail. Surely the same principle ought to be extended to the more chronic affections, which as well as the more rapid ones, have a period when remedies may be used with success, and on the expiration of which, organic changes ensue, not indeed immediately fatal, and sometimes permitting life to be extended for years, but changes which no subsequent treatment can discuss, and which surely, although tardily, cause death. We have examples of this in diseases of the heart, of the lungs, of the ovary, and, I think, of the stomach too.

Dyspepsia is a very common disease, and consists so frequently in a slight and temporary derangement of the digestive function, that we are apt to forget that it may not be always so. We find a patient of sedentary habits complaining of pain in the stomach, bad appetite, flatulence, and constipation, and we order, without much discrimination, our magnesia and rhubarb, or assafœtida pill, or decoction of quassia. The patient is relieved by the remedies administered, but the ailment returns, and the remedies again remove the uneasy symptoms, and the process is repeated, till, perhaps, the patient and practitioner tire of each other; and when they meet again the case is nearly hopeless. Let us be more attentive in watching such symptoms, and more discriminating in the employment of remedies for them; we shall then

see our treatment more successful, and we shall have fewer cases of incurable chronic disease.

The case I have related is an additional illustration of the remarks of Dr. Abercrombie, in his valuable essay on this subject, in the 78th number of the Edinburgh Medical and Surgical Journal. His description of its insidious progress is particularly deserving of attentive perusal.

The abdominal pulsation in emaciated individuals is a symptom well known to practitioners; and I would not have noticed it in my narrative of the case, had it not been somewhat peculiar in this instance. That it was so, I am confident, not so much from my own observation, as from the decided opinion of two friends, who visited the patient, and who are both distinguished for their accuracy as observers.

REVIEWS.

Quidquid venerit obviam, loquamur
Morosa sine cogitatione.—*Martial.*

ART. XI. *Principles of Dental Surgery, in two parts,* by LEONARD KOECKER, Surgeon Dentist, Doctor in Medicine and Surgery; Member of the Medical and Linnæan Society, and of the Academy of Natural Sciences of Philadelphia, &c. &c. London. Octavo. 1826.

THERE are but few who are fully aware that the physiology of the human mouth and teeth, and the diseases incident thereto, are subjects in which are involved some of the most mysterious and complicated phenomena that are presented for our study, and the exercise of human sagacity and skill—Moreover, there are, comparatively, but few who consider these subjects as possessing a degree of importance sufficient to render them objects of serious attention, or of critical investigation by any one, with a view to their relative influence upon the human body, whether in health or disease.

They are such, however, as have invited the most scrupulous research, and on which have been bestowed the labours, not only of professional dentists, but, of the medical professor—the philosopher—and the naturalist; and with a degree of zeal and success, that has long since left us but little to hope for, or expect by way of improvement, either in theory or practice. Yet almost every year evinces, by the publication of some new book, the spirit of inquiry and research upon this interesting branch of medical science; and from each of which some new and valuable hints and facts are derived to assist in the management and control of the diseases incident to that part of the human body.

As such, we are pleased to introduce to the notice of our readers, the above work; and we the more readily assume

this duty, as the author was but recently a resident citizen amongst us, and well known in his professional character.

The work is divided into two parts—in the first of which, the author commences with "general observations on the present state of Dental Surgery," &c.

As the following passage is of some notoriety, we shall offer a few remarks with a view to correct its unfavourable tendency.

"The practical part of this important department having been so long in the hands of *illiterate* men, *incapable themselves* of writing, we have been obliged to depend for our theoretical principles, and for all that was to be learned, *otherwise* than by *practice* and *experience*, upon the information of writers *not practically* acquainted with the subject. Hence many pernicious and erroneous theories and opinions have prevailed, owing to the *authority* which has attached to the names of such persons, while very little substantial and useful scientific knowledge is to be obtained from their writings," &c.

If the above remarks had been written in America and really intended for those who assume the title, and are actually engaged in the profession of a dentist, we should not have felt the same degree of surprise, nor should we have considered them quite so unreasonable and unjust. But as it is, we cannot help expressing our regret that Mr. K. should, not wilfully we hope, but through a want of more and *better information*, have justly exposed himself to severe and merited animadversion, by publishing to the world what would appear but little less than a premeditated insult upon the characters of a number of men, eminent for their *literary* talents, their professional acquirements as dentists, and for the signal services which they have rendered to this important branch of medical science. If Mr. K. by saying that we have been obliged to depend "upon the information of writers not practically acquainted with the subject." had a reference only to medical writers, from the days of Hippocrates, to those of John Hunter inclusive, we should have been ready to join issue with him in the concluding remarks of the sentence.

But why leave out of view a Fauchard, who is justly denominated the "Father of Dentists?" The first to systematize this branch of medical science—whose writings (though not without errors) after a critical examination, were sanctioned by some of the most eminent medical men of the age, in which he lived; viz. Winslow, Hequet, Finet, Helvetius, Silva, Jussieu, Bourgeois, Mouton, Chauvet, Routhonet, Mothereau,

Bertrand, Verdier, Morand, De Vaux, Tartanson, Duplessis, Sauré, De Grammond, and Laudmiey, and moreover by the approbation of the king—a work that may be considered the ground work of all subsequent improvements in dental knowledge, being replete with correct pathological views and practical knowledge; in fine, a work that will continue to be referred to and quoted until the current of ephemeral essays shall be merged in the ocean of oblivion. Why leave out of view, or withhold the merits due to the works of a Bunon, a Bourdet, a Mouton, a Tenon, a Lecluse, an Auzèbe, a Roux, a Geraudly, and many others, whose writings are replete with valuable physiological—pathological and practical information in *every* department of "dental surgery," and with respect to more modern works, we are still led to ask—why leave out of view the works of Hebert, who had long anticipated the pretended and important discovery of Dr. Blake(of Edinburg) respecting the natural history of the teeth? —The *invaluable* works of Fourdain, which, in point of physiological, and especially, pathological knowledge, have, and do still stand unrivalled? The works of Garriot, Laforgue, Duval, Maggiola, Baume, Mahon, and a number of others, who are mostly acknowledged members of the college of Surgery at Paris, and elsewhere? and whose writings contain an invaluable fund of information in every department of this branch of surgery, and such, we may venture to say, as "that he who runs may read, and the wayfaring man, though a fool, cannot err?"

To these questions we are unable to answer, unless it be that they are continental writers, and mostly French. Or that the work would not be so well calculated for the meridian of London.

It is true that Mr. K. says, that "The French, particularly, deserve our acknowledgments for their industry to increase the stock of our dental literature; yet they have not produced *one* original standard work of the natural history of the teeth, similar to those of Hunter, Fox and Blake, which could furnish them with sufficiently just and solid principles for the innumerable speculations and theories which they have advanced."

It is true that, with the exception of Jourdain, their works have not been so highly and extensively embellished with plates, descriptive of the natural history of the teeth; but we will venture to assert, that they have been far more explicit in every part connected with the natural history of the teeth, than either Hunter, Fox, or Blake, with the exceptions of

the natural history of the teeth of animals; and if works on science are not to be understood, unless embellished and illustrated by plates, what becomes of, or of what use would be very many of our anatomical, surgical and medical works?

On the subject of quackery, we most heartily agree with the author; and, moreover, do not hesitate to declare, that its almost universal prevalence is a circumstance not only deeply to be deplored, but one that has, and still tends to enchain the profession in disgrace: for such is the unpardonable want of principle in many who proudly rank themselves with the most skilful; such the aggravated injuries and irreparable mischiefs that are inflicted upon many of the incredulous of our own country, that in many parts, all confidence is lost; and the word dentist has, in numerous instances, become a term of reproach. But we "do hope and believe, that there will not in time to come, be wanting," men, whose province it is to move in the higher branches of medical science; who are, or *ought* to be competent judges of the utility and importance of "dental surgery"—and the necessary pre-requisite qualifications, indispensable to the honourable and faithful discharge of the duties of the dentist: men whose zeal in the cause which they have espoused, and whose lives are devoted to the sole object of really endeavouring to ameliorate the condition of their fellow creatures, by alleviating the pangs of disease, and securing if possible, in perpetuity, by successful treatment, one of the greatest, most useful and ornamental parts of the human body; will, one day, be able to rescue the profession from the disgrace and opprobrium that is at present attached to the innocent and meritorious, as well as to the really unprincipled and guilty.

Whilst we thus freely express our opinions of the glaring impositions that are too frequently practised by many of the profession, we cannot withhold our expression of disapprobation of the unceremonious manner in which some authors endeavour to underrate the abilities and skill of most of their professional brethren, by a copious effusion of defamation and abuse. This was, in no small degree the case with the celebrated John Bell, who, it seems, "remembered to forget," that he himself *was once a tyro.*

In the course of the animadversions of our author on quackery, or the "present state of dental surgery," he observes, "where errors so greatly affecting the health, the happiness, and even the life of a human creature, are the result as they frequently are, of an unpardonable ignorance of the most common principles of medicine and surgery,

they call for unqualified reprobation; indeed I do not scruple to say, that a man who pretends to practise this art without a better acquaintance with surgery and medicine than is generally deemed necessary, ought to be made deeply responsible for the consequences of his presumption."

In these remarks, though expressed with a seeming severity and harshness, we may say, with some degree of confidence, that they are not altogether undeserved nor uncalled for; yet we must add, that were these denunciations to fall indiscriminately upon all those who have committed faults in the commencement of their professional career, there are many who, although they may have arrived to a moderate degree of eminence, on a retrospective view of the obstacles and difficulties which impeded their progress, whilst, in their unassisted efforts, they were endeavouring by what they considered a legitimate course of procedure, to merit the confidence and patronage of a community, would look back in dread, dismay, and self gratulation, at their escape from such unsparing penalties. In commenting upon the abuses practised in any art or profession by persons ignorant of, or incompetent to the undertaking; it is of but little avail to pour forth a torrent of invective against them, and more especially for any one who is habitually and successfully engaged in the same pursuit; for in most cases, the persons to whom those censures are intended to apply, generally view them as the result of a selfish policy, and intended only to heighten the glare of the author's or writer's own personal fame, by endeavouring to prove that but very few besides are possessed of the qualifications necessary for the undertaking.

This serves only to embolden and incite them to a determined prosecution of their own views, regardless either of consequences or character.

Mr. K. observes that empiricism is an evil which is not to be overcome by argument, expostulation, or entreaty. In this we perfectly agree. And we will further add, neither is it to be overcome by censures or by threats. The only means by which so desirable an object can be obtained, and "dental surgery" placed upon that respectable footing to which it is justly entitled, is, in the first place, to obtain the *confidence*, the *approbation*, and *sanction* of the members of the medical and surgical faculties. Secondly, for such as do possess the confidence and approbation of a sufficient number of members of that honourable body, to form themselves into a social and *legal compact*, with *powers* to *examine*

and grant diplomas to such as may become candidates for the profession of a dentist.

Then they may go forth justly entitled to respect and to the confidence of a liberal community, who if they prefer hazarding their comfort and welfare in the hands of a person without principles, without talents or skill in his profession, must charge themselves with the consequences.

In pursuing the remarks of the author on the "present state of dental surgery," and in which there are many judicious observations, as well as glaring inconsistencies, we notice the following:—"But one of the greatest and probably, one of the most mischievous errors even among dentists of some celebrity, is, the common practice of inserting artificial teeth, before the mouth has been properly prepared for their reception, by being brought into a healthy state." P. 9.

We regret, to find this sentence so indefinitely expressed, and more so, on seeing the uncharitable and unjustifiable imputation, contained in the first paragraph of the following page, viz: "For to suppose that the operator was fully sensible of the consequences, the great suffering, and inevitable destruction to the teeth, and to the general health of the patient, were to suppose him something *worse* than a deliberate destroyer of his species by secret violence." P. 10. This is indeed, a language which we consider better suited to a description of the sanguinary myrmidons of a Robespierre than to grace the pages of a scientific work.

But with respect to the indefinite manner in which the passage alluded to was expressed, we will observe that we believe that in all cases where artificial teeth, or natural teeth, artificially to be set, are required, some preparation is, in almost all cases, necessary. But are we to suppose that if a lawyer, or a divine, the one having on the following day an important case to argue, the other a sermon to deliver, should, by accident break off, or out, a couple of decayed teeth in the front of his mouth, which from inattention or neglect, is not in perfect health, that either one or the other are to wait one or two weeks, before the accident can be repaired, and the difficulty of articulating obviated, in order that the mouth may be "brought into a healthy state?" Are we to suppose, that if a gentleman is going from London to Edinburgh to be married, and, by accident, the half decayed bodies of a couple of front teeth are broken off, the very day before the nuptials are to be celebrated, is he to wait in "durance vile," two or three weeks in order that his mouth,

which is not, perhaps, in the condition in which it *ought* to be, on such an occasion, should be "brought into a healthy state?" If so we should, indeed, under such circumstances, think it a very hard case; and one in which very few of the dentists of London or Edinburgh, however eminent they may be, are so fastidious as not to repair the injury immediately, unless prevented by imperious circumstances, *knowing* that no unfavourable consequences *could result* from the operation if judiciously performed. In this we are supported by incontestible facts that almost daily occur.

In the subsequent remarks on artificial teeth—on the extracting of teeth—and "the stoping of teeth with metal"—we are pleased to say that there are some judicious observations. But in a number of instances we are led to question the correctness of the principles stated—and moreover, in others we cannot withhold our disapprobation of the loose and indelicate manner in which they are expressed, (viz:) of artificial teeth—"If such are the consequences of these operations when *skilfully performed*, what must be expected to be the results of bungling operators in the same circumstances." (P. 11.)

Bad enough we have no doubt. But are we to understand or believe, in the present advanced state of "dental surgery" (for we are sure that it is very far advanced, in many parts, both on the continent of Europe, and in England,) that the insertion of artificial teeth has been uniformly attended with those disagreeable consequences, even when *skilfully* performed, when thousands can attest that they have worn and used them for ten, twenty, thirty years and longer with the most *perfect ease* and comfort, and free from the alleged consequences? We can easily imagine that unpleasant effects have arisen from this source, and we can as *easily* imagine the *causes*, and we hope our friend will, with his usual frankness, state the principal ones as well as that of the consequences.

Again, in the extraction of teeth—

"It is the *destruction* of *very important* parts of the human body, and if injudiciously performed, its destructive consequences not only injure the appearance of the mouth, but also *undermine* the constitutional health of the subject.—While, on the other hand, the removal of diseased and painful teeth, and dead roots, by *cautiously* and *judiciously* performed operations, is, frequently, a most powerful and certain remedy, not only for many painful diseases of the mouth and teeth, but also for the most obstinate diseases of the

general system." (P. 12.) To the last sentence of the paragraph, we give our most cordial assent. But how is it, that from time immemorial, this operation has been performed upon twice told millions, by every denomination of artisans, handycrafts-men—and even in some instances by the *sufferers themselves*, without any unpleasant consequences, either to life or health? Our author states that he has, himself, often been engaged in the "destruction" of these "very important parts of the body"—by extracting upwards of twenty teeth at a time, (see p. 110 and 11,) and that with the most salutary results. Indeed without encountering any serious difficulties in the solution of this problem, we think that the only inference to be drawn is, that the operation is no where to be *judiciously performed* but in Conduit street, Hanover square, London.

Again—"I am sorry to observe that the abuses to which I allude, are *not confined* to the practice of the inferior (inferiour) orders of our body, but that their effects are, very frequently to be observed in the practice of dentists generally considered of the *first rank*, and not only of those highly esteemed by the public, but of such as are held of good reputation even by medical men." (P. 15.) Further—"In France, where it cannot be denied, that general surgery has made great progress towards the attainment of excellence, the *abuses* and malpractices of dental surgery, here referred to, are *quite* the *order of the day*." (P. 16.)

In considering the first sentence, we must, in the first place, confess that we have to deplore the apparent want of a discriminating judgment, and the misplaced confidence of the public, and more especially of the people of England, and secondly, we as deeply sympathize with, and deplore the uncourteous manner in which the dentists "of the first rank," have "even in England," (see p. 17,) been saluted in the present instance.

We have always believed, and not without *just reason* too, that there were gentlemen of the profession in London, Liverpool, Dublin, and Edinburgh, eminently qualified, both theoretically and practically, for every part of "dental surgery" and men whom, in the language of our learned friend Dr. M. we should be "proud to shake by the hand."

But if in these cases the representations be true, and if such indeed be the facts, we are somewhat disposed to believe that the instruction given to the accusers of the woman taken in adultery, would not have been altogether inapplicable. As to the abuses and malpractices of "dental surgery"

in France, being 'quite the *order of the* present *day*,' we are willing to admit that there are charlatans in that country as well as in America, Germany, or London; but we must consider the charge as a libel upon the characters of some of the most skilful and efficient dentists in every part of the world—men whose researches in physiology, pathology, and practice of "dental surgery," have outstripped those of every other country—men whose works do honour to the profession—works which, for the skill and ingenuity with which they have been executed have been viewed with mingled surprise and delight; and lest any one should doubt the correctnes of our remarks, let us inquire, where is the country upon earth in which more attention has been paid to the subject, it having been always considered by them as of primary importance to health and to beauty?—Where is there one that affords greater facilities to the acquisition of perfect knowledge of every part of it? In fine, where did it originate, and receive its first impulse? We answer, in France.

Before Thomas Berdmore, who was dentist to his majesty, George the 3d, in the early part of his reign, wrote his practical treatise on the teeth, (and which, by the by, contained as much useful information to a dentist as almost any work since published in England,) there had been volumes published in France embracing, even at that time, facts that have since been copied into English works, and proclaimed as new discoveries; without ever having acknowledged the real source whence they were taken. But we shall wave a further discussion of the subject, presuming that it is too well known, to need any further elucidation.

Chapter 2d.—"Of the general prinicples of dental surgery." As to the preliminary remarks in this chapter and the principles inculcated in the several articles, or sections which it contains, we are disposed to sanction them in the most cordial manner; for although they present nothing new, they contain the fundamental principles of a correct knowledge of this branch of medical science, and ought to be well understood, and as well attended to by every person who is disposed to embrace the profession of a dentist; for without an intimate knowledge of those subjects, he may be compared to a ship at sea, without a compass or rudder.

Chapter 3d.—"General remarks on the natural history of the teeth &c." In this chapter, the author has attempted a brief outline of the natural history of the teeth, with a few pathological remarks. But as it is a subject on which almost every writer on the physiology and pathology of the teeth.

has given his views, we can scarcely expect to find any thing peculiarly interesting or new. His pathological remarks, however, clearly evince a more than ordinary attention and application to the subject, and are judicious with a few exceptions. For example: In speaking of nervous sympathy he observes:—

"In consequence of this sympathy, it often happens when a single tooth is painful, that the whole side of that jaw is affected, and sometimes even the *opposite side of the same jaw*," &c. p. 41.

To this we must give a decided negative. In no instance, we believe, where the disease and consequent pain of a single tooth is idiopathic, did the teeth of the opposite side of the same jaw ever manifest symptoms of pain from sympathy; and for very obvious reasons. Our author needs not to be told that the teeth upon each side of the jaw are organized by two distinct sets of nerves, branches of which are thrown off, to organize each tooth from the densaspientia to the symphysis of the chin, *and no further*. It is the same in either jaw. Hence a diseased or painful tooth, frequently occasions a complete hemicrania, whilst the opposite side of the head is not, in the least affected. Hence too, from a corresponding economy in the system, one half of the body is completely paralized, whilst the other enjoys all its faculties.

If pain exists in the teeth of both sides of the same jaw at the same time, it must arise from one of two causes. 1st. Teeth may be diseased and their nerves exposed on both sides of the jaw at the same time, and the same cause which produced pain on one side may occasion pain in the diseased tooth of the opposite side. 2dly. It may be symptomatic, and depend exclusively upon some general disease, or peculiar condition of the body.

Chapter 4th.—"General remarks on the diseases of the teeth," &c. In modern days, it is a very common occurrence to hear the following questions asked. What can be the cause of the uncommon prevalence and premature decay of the teeth, and their consequent diseases?

In answer to these questions we will observe, that Dr. Beddores, in his writings on consumption, has done much in bringing into view, and explaining the remote causes of many of the diseases of the present day, as well as that of consumption, and all of which causes have a greater or less influence in promoting the destruction of human teeth, and their consequent diseases.

Mr. Garriot, and some other French writers on this sub-

ject, have come more immediately to the point, by advancing an opinion which nearly corresponds with those of our author, and to which we yield our entire acquiescence, viz: "to the present luxurious habits of man, and his artificial mode of living." P. 43.

To ascertain satisfactorily, the remote and proximate causes of the premature decay of teeth, is a very interesting and important point; not so difficult we presume, as is generally supposed; and we regret that Mr. K. did not enter into a more minute and critical investigation of the subject. We are, however, gratified with the manner in which he has introduced it, and with his views and manner of treating it thus far.

Chap. 5th.—"Of constitutional diseases and their effects on the teeth." In this, the author endeavours to point out the influence which diseases of the general system exercise over the teeth, and, more especially, those of an inflammatory grade; on the subject of which he observes,

"After death from violent fever, I have found the blood in the blood vessels of the lining membrane of the teeth sometimes very dark, so that the external appearance of the teeth was quite red." (P. 51.)

The author here, as in most instances, substitutes the term "lining membrane of the teeth" for the nervous pulps. We are perfectly aware that the canal or cavity of each tooth, has a lining membrane of the most delicate texture, and which is nothing more than a duplicature of the external periosteum of the tooth sent down to form a kind of aponeurotic expansion or fascicula which envelops the nerves, and forms the medium of union of the nerves, to the bony canal in each tooth. Hence, in speaking of the nerves of the teeth, we are not to be understood as having any reference to the *lining membrane*.

We are ready then to agree with the author, that in several diseases of the body, but more especially in those of a highly inflammatory grade, the vessels that organize the teeth, and the nervous pulps, do assume this dark and inflamed appearance; and moreover, it is sometimes produced by very active medicines, and when the disease for which they were given is not of a highly inflammatory kind. In this condition, and in the *living* subject, teeth of a certain description as those that are young, very thin, and transparent, a difference of colour may be observed by a *very* close examination. But no grade of fever, nor medicine however active, has, we believe, ever been known to rupture the blood vessels, and inject the vascular portion of the bone of the teeth with

blood. We have observed, in a *living* subject this appearance may be detected. But as, in death the blood mostly leaves the vessels in which it circulates, we can detect no appearance of the kind after death. Hence, in a bushel of teeth that have been taken from subjects that have died of *all kinds* of diseases, no *real* appearance of the kind can be detected, if the teeth are properly freed from the extraneous matter, which will sometimes give them a false colouring. The real difference of colour which may be seen amongst so many, depends on circumstances which it is not necessary here to mention.

"*On constitutional changes.*"—The author observes, "during pregnancy, for example, the diseases already existing make a much more rapid progress than at other times, and even new diseases take possession of the sound teeth and gums, in consequence of the increase of excitement produced by such a change." (P. 56)

This is a fact so well established, that we do not hesitate to say, that in no period of female existence can the teeth be said to be in a more critical situation, or in greater danger of destruction, than during the period of gestation.

At the same time, we do not agree with the author that it depends on, or is a consequence of the increase of excitement produced by such change. It is a very common thing to see similar effects produced upon the teeth of females, after the suppression of the lochia; where, we will venture to say, there is rather a want of excitement, than otherwise. The real cause therefore, seems to have been overlooked, or not to have been understood.

Of Diet or Nourishment—Of Climate—Of Medicine, &c.—On pursuing the remarks of the author, on these subjects, we should be led to say he has become familiar with the writings of Laforgue, and some other French authors, but more especially with those of Baume. Whether so or not, however, it is a subject not sufficiently attended to, and we would gladly see it more frequently discussed with the same degree of frankness.

Of the Diseases of the Mouth.—Under this head the author has attempted to point out some of the causes of the diseases of the mouth, amongst which is, first that of tartar; on the subject of which he observes,

"Tartar is one of the most frequent causes of diseases of the teeth. It acts *directly chemically* in the destruction of them," &c. (P. 65.)

It is generally supposed that all kinds of deposites and discolorations upon the teeth, are nothing more nor less than

tartar. Now there are four distinct kinds of deposites observed upon the teeth; the three first of which are, first an orange colour, and not common; the second is of a black or brownish black, very common; thirdly, a greenish deposite, mostly prevalent in the teeth of children under fourteen years of age; and is highly injurious to the teeth. Of the properties, qualities, or effects of these, we have nothing to say at present, only that they have nothing to do with tartar.

The fourth, which is commonly called tartar, and by some (very properly we think), tufa, is that, we presume, which the author has reference to. It is almost universally believed that this substance is the most powerful agent; the principal cause of the decay of human teeth. That it is highly mischievous in its consequences, and "indirectly" causes the loss of millions of teeth, we will readily admit: but that it acts *chemically* in the destruction of them, or has any direct agency in causing the *actual* decay, or caries in the teeth, is what we unhesitatingly say is incorrect.

From some experiments on the tartar of the teeth by our friend, Dr. M——, (see Med. Repository, vol. 2, p. 425,) it was taken for granted that this substance was a septite of lime; consequently that it, with the uncombined septic acid, formed in the mouth, was one of the causes of the uncommon prevalence of decayed teeth in the "Fredonian States." Hence he observes, "Alkaline dentrifices would therefore appear capable of *removing* the calcareo-septic incrustations from the teeth, but incapable of corroding the teeth themselves:" (Med. Repository, vol. 2, p. 430). A wonderful discovery indeed! But we would verily ask if any person *ever* found a real caries *under* an incrustation of tartar upon the teeth, either of recent formation, or of ever so long standing? Oh yes! many will say; and the cavities, sometimes large and sometimes small, *completely* filled up with tartar. But we will beg leave to ask another question? Will any experienced dentist, after being satisfied that such cavities are filled up with tartar, ever attempt to remove it with a view to fill the cavity with any other substance? We answer no! unless he wishes to increase the amount of his fee by so doing, or does not understand the subject; for certain it is, that no kind of plugging or stopping can be made *more perfect* or *more lasting*. Of this we have numerous and incontestible proofs, both in the teeth of the living, and in those of the dead that have been exhumated long after their interment. Where then are the evidences that "it acts directly chemi-

cally in the destruction" of the teeth? We believe no where, except in imagination.

Secondly, "*of Saliva*."---On the subject of which the author observes, "the saliva when in a degenerated or morbid state, forms likewise, an active cause of disease of the teeth, by its permanent chemical influence upon the parts of the mouth generally, as well as by the greater predisposition to the deposites of tartar possessed by the fluid," (p. 66.) We are perfectly aware that it is no uncommon occurrence to hear medical writers and others, speak of the morbid condition of the saliva. Of acrid saliva, &c. &c., and of its injurious effects in the mouth, in various ways. This is not so much a matter of surprise, as custom may have sanctioned the legitimacy of the expression. But when we hear one, whose province it is to inculcate correct views of the human mouth and its diseases, which can only be done from a correct and critical knowledge of its physiology; we have a right to expect a language more in accordance with truth, and a little more expressive of a scientific knowledge of the subject. Pray, let us ask, what is saliva? (Reader do not be offended.)

Why, we all know, that it is a fluid that flows abundantly into the mouth from numerous glands, destined for the *sole and only purpose* of *elaborating* this fluid. And is this fluid susceptible of any change? Was it ever known exclusively, to assume a morbid consistence; to become acrid and corrosive in its qualities? Was the lachrymal secretions (which we consider as differing in no degree from saliva), ever known exclusively, to manifest these qualities? We answer no! From the most scrupulous attention to the subject, we have *never* been able to discover any change in this fluid, either in health or disease. But it will be observed, we often witness a very material difference in its consistence—in its qualities—and in its effects. How is this to be accounted for? We answer that it is by the *admixture* of the mucous secretions from the whole internal surface of the mouth; and which secretions are susceptible of the most acrid and corrosive qualities; so much so at least, as to corrode silver, as salt water does iron. It is the morbid or acrid state of the secretions that corrodes and inflames the coats of the eyes, and marks the course of the tears down the cheeks of some persons. It is this that frequently occasions inflammation and soreness in the mouth, and more especially in the gums, where there are no signs of tartar to be seen upon the teeth.

The effects of these vitiated secretions are often experienced by persons who, going to bed with a slight fever find, on

awaking in the morning, an unusual quantity of depraved mucus in the mouth, and having a disagreeable taste. In a short time, however, and as soon as the saliva begins to flow, this unpleasant taste in the mouth is removed without any exertions, and the person again feels comfortable.

How is this to be considered a morbid condition of the saliva? If so, is it likely that it has the power to correct itself? As likely, we presume, as that of tainted meat, or bad wine.

It is true that *some* of the professional writers in France, (where "abuses and malepractices are quite the order of the day;)" speak of vitiated or acrid saliva as one of the causes of the decay of the teeth. Others again, who have, doubtless, taken a more correct view of the subject, allege that the decay of the teeth is occasioned by a "corrosive sèrositè," that is retained about the teeth. Others, of a corrosive lymph; and moreover, of a saliva that is saturated with the morbid mucus of the mouth, by which the teeth are destroyed.*

Indeed it is, in almost all cases, to the vitiated condition of the mucous secretions, that we are to impute the disagreeable consequences alluded to, and not to the saliva; and it is greatly to be desired, that, in all cases when speaking of a vitiated saliva, a proper distinction should be made; and more especially by those who attempt to treat of the diseases of the mouth.

"*Of external remedies.*"—In the remarks under this head, there are a few observations worthy of better company. In general, however, they are so exaggerated, and, at the same time, so pointedly directed to one grand object, that, with one or two exceptions, we are disposed to leave them to the consideration of those who can digest them with a keener relish. The remarks of the author on external remedies, are introduced by the following:

"Tooth powders, tooth pastes, tinctures, essential oils, and mixtures of *all kinds*, *&c.* (&c. is superfluous), employed without *proper* judgment, are so many causes of diseases of the teeth and gums."

"All kinds of dental operations, performed either in an injudicious or unskilful manner, even the improper use of *tooth brushes* and *tooth picks* not excepted, are like other causes of irritations, and are consequently productive of *divers* diseases of the teeth." (P. 67.)

* See Auzebé, Courtois, and Laforgue.

We shall forbear commenting upon "mixtures of all kinds" that are indirectly objected to, and merely notice those of "tooth powders, and tooth pastes."

Had the author observed that these articles, when *selected*, and employed "without proper judgment," are attended with consequences prejudicial to the teeth, we should have been ready to yield our assent to the correctness of his remarks. But are we to understand that there are *no* tooth powders, tooth pastes, &c. but what may be considered as "so many causes of diseases of the teeth?" Or are we to understand that, although there may be *certain kinds*, to be had of a certain person, at a particular number, in a certain street, &c. that are good and safe; yet, if not used agreeable to certain instructions, and with "proper judgment, are productive of bad consequences?" Indeed, this seems to be the most plausible inference to be drawn, not only from the premises, but from several circumstances; for the author, in a subsequent part of the work, in treating "of the preservative treatment of the teeth and gums," observes, "the means I have used are, sometimes *tinctures* and *mixtures*, but principally *powders* and *brushes*," &c. &c. (see p. 184.)

Again—"to suit the many varieties of cases, *I have* always found it necessary to be provided with a *great assortment* of *powders* and *brushes*, with many of the latter in such a state as to be capable of receiving *any* shape, to suit special and unusual cases." (See p. 185.)

With respect to tooth powders and tooth pastes, we are perfectly aware that there are various kinds that are extremely hurtful, and such as ought never to be used. On the contrary, we are as well aware that there are various kinds, compounded with *real skill* and *judgment*, and such as have been used for twenty, and even thirty years, with the most salutary and beneficial effects to the mouth and teeth of thousands, and that too without any regard to "proper judgment," or any particular instructions as to the manner in which they were to be employed.

Why then this sweeping denunciation, not only against "tooth powders, and tooth pastes," but even against "mixtures of *all kinds*."

Perhaps the objection rests upon their having been employed "without proper judgment"—and in what, pray, does this consist? Is there any, until now, unheard of method—any art of legerdemain, or secret mystery in the application of a dentifrice, or tooth paste to the teeth? If so, 'tis passing strange. We now pass on to tooth brushes, and tooth

picks, in hopes that a proper use of them will remove some of the redundancies.

It is somewhat singular that an article, apparently so trifling, (though nevertheless of much importance, should have elicited, from various sources, such copious remarks, and at the same time such a contrariety of opinions ; many of which are more calculated to excite our commisseration for the imbecility which they betray, than for any good or evil which they can ever do.

Some are of the opinion that tooth brushes should be made short, some long, some broad, some narrow, some single, some double, some flat, some round, some strait, some curved, some soft, some very stiff, and some of an intermediate kind.

As to their uses, some say they must be applied, for instance, in the superior jaw, to the gums, and rubbed downwards towards the points or edges of the teeth. And on the inferiour jaw, from the edge of the gums upwards towards the cutting edges of the teeth--some direct that the brush should be used with a rotary motion—others that it should be "transversely, up and down,"* &c. &c. But we are fatigued with this tirade of caprices.

It may be desirable, however, that this apparently *knotty* subject, should, if possible, be elucidated, and that the diversity of opinions should be reconciled upon some principles that have, at least, a slight claim with truth.

To accomplish so desirable an object, would, doubtless, be gratifying to many. With this in view, we will hazard a few remarks.

It has been observed by Sir John St. Clair, in his essay on longevity, we believe, that "tooth brushes were made for the gums ;" and, perhaps, he never spoke with more truth.

The use of tooth brushes, we all know, or ought to know, is two-fold—1st, To remove the incrassated mucus and remains of food from the mouth, and amongst the teeth to keep them clean, then for preventing their tendency to decay ; but the most important part is—2d, To keep the gums in a healthy condition. In the latter, consists the important and highly beneficial use of tooth brushes ; and to effect this it is immaterial how, or in what manner the brush is applied to the gums ; and without going into an elaborate disquisition of this subject, which would require a correct physiological view of the mouth, and more especially of the gums and their

* See Longbothams treatise on the teeth.

offices, we will venture to rest the issue upon one simple illustration.

We trust there are but few dentists of experience and *observation*, who are not able, the moment they look into a patient's mouth, to tell on which side, or on what point the person masticates his food, and this *only* by the comparatively cleanly state of the teeth, and the firm and healthy appearance of the gums; whilst in numerous instances, the teeth of the opposite side of the mouth, are invested with a covering of tartar and sordes, the gums inflated and puffed up, and, in many instances, almost in a state of sphacelus.

And to what, pray, are we to attribute this marked difference in the two sides of the same mouth? We answer, to the simple *mechanical action*, solely and exclusively, of bread, meet, &c. &c. masticated three times a day. This fact may be doubted; but we contend that it is in accordance with a universal law of nature; viz., that where a graduated mechanical action is applied to a part of the human body, and even of animals, the part uniformly becomes more firm and hard; witness, not only the gums, but the hands and feet, and especially the gums of aged and toothless persons. Admitting this part so far as relates to the gums, and which cannot be controverted, we would seriously ask whether a "proper judgment," or any rigid or particular method, is essentially necessary in the application of *food* to the gums? Is it as necessary that the food in mastication should act transversly upon the teeth; or from the gums upward and downward towards the cutting edges, instead of being applied on the cutting and grinding edges, and pressed upon the gums? If so we shall exclaim, Patience to him who has a craving appetite!

From these facts, it may easily be seen, that a "proper judgment," is not so essentially necessary in the use of toothbrushes, for it is immaterial how they are applied to the teeth, or in what manner they are used, so that thy are not *too stiff*, and act freely upon the gums in *every part* of the *mouth*, for on the firm and healthy condition of them depends, in an eminent degree, the preservation of the teeth; why then this apparent disposition in many to rack their brains in fruitless efforts, to find out some peculiar method of applying a toothbrush to the teeth, and of moving it after it is applied; and, above all, to hold out the idea, that there is some mystery in the business, a knowledge of which is only to be obtained from the source whence it emanates.

The same observations will apply, in a certain degree to

tooth picks; on the subject of which we are impelled to offer a few remarks for this simple reason, that we entertain a high opinion of their importance and real utility; and we would seriously ask, how the use of this simple article can "act like other causes of irritation," and be "productive of *divers diseases* of the teeth?" although we can imagine in what way it may *possibly* do a trifling injury. We will leave it to the author to solve the question.

We are not disposed to dwell long upon this *weighty* matter; but we will, with as much brevity as possible, explain our views on this subject, that the reader may judge whether the use of tooth picks are prejudicial or not.

It it generally supposed that the principal, or only use of tooth picks, is that of removing the remains of food from amongst the teeth; but this is an erroneous opinion. We have shown, or endeavoured to show, that a certain degree of *mechanical action* or *friction*, is indispensably necessary to a healthy state of the gums, no matter whether performed by hard crusts of bread, sea biscuit, beef-stakes, or a tooth brush. Now, we have shown that the latter has a most salutary effect in this way. But however carefully or generally it may act upon the gums; there are parts on which it cannot be made to act *properly*, no matter what the kind or form. We mean those parts of the gums which occupy the interstices of all the teeth, passing through from the external to the internal gums. The situation of these parts is such, more especially in young and middle aged persons, that the depraved mucus of the mouth, the remains of food, and, moreover, a small quantity of tartar, insinuates itself between the teeth and under these portions of the gums; by which means they become compressed and irritated; the free circulation is disturbed; they are elongated, swollen, flabby, and bleed profusely on the slightest touch, (so much so we are informed, that dentists are frequently under the necessity of cutting away these diseased parts, that healthy granulations may occupy their places,) whilst the gums below, on either side of the teeth are, apparently, in perfect health.

Now there is no method so well calculated to obviate this state of things, to restore those parts to health, and preserve them so, as that of the use of a tooth pick, either of gold, silver, or one made of a common quill; the latter of which we prefer to any other kind, and would recommend it to be used in the following manner, viz: There being a natural opening between every two teeth in the mouth, if they stand regular, the point of the tooth-pick should, at first, be

gently introduced between the teeth above the elongation of the gum; as it passes in, meeting with resistance by the junction of the teeth at their cutting or grinding edges, it is forced down (in the inferior jaw, and contrary wise in the superior jaw) upon the gums. If they are in the smallest degree diseased, blood is immediately discharged, and if so the oftener they are made to bleed, in this way, the better; for by this means the blood vessels, which at first were turgid, are relieved, the cause of morbid excitement is overcome, and in a short time, by a habitual use of this valuable article, in the manner described, every part of the gums may be restored to perfect health. In this state, if the tooth pick is pressed upon the gums, between the teeth, twenty times a day, no blood will be seen to follow, and this simply by the *mechanical pressure* upon the parts, as in the mastication of food, and the use of the brush. Nothing is more obvious, and nothing is more certain.

To this we will add a brief illustration.

It is well known that as man advances in age, or, we may say, from thirty years and upwards, the gums are disposed to become more firm, or to shrink away, and the teeth to become worn down, so that the food is more easily admitted between the teeth. To remove the inconveniences which this always occasions, the tooth pick is employed, and at last becomes not only necessary, but is habitually resorted to, and the consequences is, that instead of causing "divers diseases" it renders the gums almost as hard as the hands. Hence one of the causes why the teeth of persons (men more especially) over thirty years of age are so much less subject to decay, between them; for it is an undoubted fact, that in proportion as the gums are suffered to remain in a state of disease, so are the teeth disposed to decay.

From this imperfect and superficial view, we trust that we are justified in recommending tooth picks, (when used in the manner above described,) as one of the most useful and salutary means of preserving the teeth from decay, and the mouth in a healthy state, that can be used; and as such, ought to be an appendage to *every ladie's toilet*, and in *every gentleman's pocket*.

If these facts are to be relied on, and we trust they may, we would again ask, why this frightful representation of *imaginary* evils, resulting from the use of means so useful, so salutary, and so indispensably necessary to the comfort and welfare of millions of beings? We say frightful representations, for we do consider them as calculated to induce

all who hear or read them, to doubt the efficacy of any means that can be used for the preservation of the teeth, unless recommended by the oracle of truth. If such there be, we do hope for more convincing proofs; for in science we expect and look for truth; but *not in fiction*.

Chapter 6th.—Treats "of the morbid effects of the diseases of the teeth, and the parts immediately connected with them, upon the general system."

In this chapter, we observe several passages that bear the stamp of truth's own seal, and from which, those who tread in the higher walks of medical science may be benefited by an attentive perusal. At the same time we witness, in a number of instances, the same disposition to magnify circumstances of an unreal nature, to a degree of importance that neither facts nor experience can ever justify.

On the various sources of morbid excitement, our attention has been directed to the following. "The teeth having been deprived of their vitality by the destruction of their lining membranes, are not only rendered *useless*, but are converted into lifeless incumbrances upon the system, and produce by their mechanical and chemical irritation, an action similar to that effected by gangrene or mortification in other bones, by means of which nature attempts to throw off the dead part." P. 94.

We are sensible that teeth of this description are frequently, sources of great irritation and pain. But we are by no means disposed to yield our assent to the opinion that teeth of this description generally, as the expression would imply, "are not only rendered useless, but are converted into lifeless incumbrances upon the system." Lifeless, it is true they are, under such circumstances. But instead of being "rendered useless," and "incumbrances upon the system," we are bold to assert, that in six cases out of nine, they are, or may be rendered, extremely useful and important to the patient in various ways; and that without manifesting any indication of "mechanical and chemical irritation." There are thousands whose principal dependence in mastication, is the remains of teeth, either worn, or decayed and broken down, and deprived of their vitality; and yet the persons so situated, enjoy the most perfect health, and are free, not only from general disease from this cause, but even from local irritation about these teeth. Moreover, we know (and we trust that the author, in the course of his *long* practice, has made himself acquainted with the fact,) that in deep and repeated salivations, or a severe and long continued course of mercury, every

tooth in the mouth, however sound it may be, is completely disorganized and deprived of its vitality. Not only so, but in consequence of the long continued excitement in, and thickening of the periosteum of the teeth and alveoli, a species of exostosis *is formed* upon the roots, which, in some instances, completely envelops the ends of the roots and closes up the orifices or foramina through which the nerves and blood vessels pass into the teeth. Now if decayed teeth and roots, deprived of their vitality, are "*rendered useless*," and are "incumbrances upon the system," we might reasonably expect that sound teeth deprived of their vitality, no matter by what means, would be equally useless, and in like manner "incumbrances upon the system." Yet it is a fact that the teeth, thus disorganized, though deprived in part of their support, by a partial loss of their alveoli, will, on the restoration of health, become firm and almost as useful as ever, and that without producing either "mechanical or chemical irritation."

It is true that decayed teeth, and roots of teeth deprived of their vitality, are sometimes liable to disease, and to much and serious mischief. But a dentist, with the necessary qualifications to constitute him such, can invariably detect and identify the cause.

It not unfrequently happens, that a person whose teeth are in a general state of decay, is attacked with severe pains in the ear, the articulation of the jaw, the shoulder, and the temple. Nay, a person may labour under some general, or even local affection of the body, which depends exclusively upon the condition of one or two teeth, although they manifest *no symptoms* of pain. If, however, the teeth should be suspected as having any agency in producing the disease, under which the person suffers, every decayed or suspicious tooth in the mouth is denounced, and directions are given to have them all removed; when, in all human probability, one or two, at most, would have answered every purpose. We know of a case in which a very eminent physician of our own city, caused twelve teeth to be extracted from a ladie's mouth, for an obstinate and unyielding affection of the stomach, leaving but one solitary tooth remaining; this was subsequently extracted, leaving her perfectly toothless; and all this with no better effect, than if he had caused so many hairs to be pulled from her head; for she continued to labour under the same complaint, until her death, which happened several years after. Hence, as teeth of this description may, by a judicious treatment, be rendered useful for life, we see no reason that can justify this inveterate hostility, or that

this war of extermination should be waged against all teeth deprived of their vitality. It is true that the author, in a subsequent part of his work, makes an exception to this rule: nevertheless, in his treatment of some of the cases which he has noted, we have reason to say, that he made no small havoc among teeth of this description, inasmuch as the number of twenty-two were removed from the mouth of one person in a short time. And, moreover, he says, "it should always be considered as a golden rule, *never* to be departed from, that *every* dead tooth and root and *all* such teeth as cannot be preserved *alive, should be extracted;* and that there should be no dissuasion, nor pretext, nor excuse of any kind made use of, to avoid the operation." P. 337.

The absurdity of such a doctrine is too evident to need any comments. We leave it for those who may require professional advice and treatment in such cases, to determine which of the two evils they will choose.

We shall dispense with any remarks on the 7th chapter, inasmuch as the exceptionable parts would lead to a discussion too lengthy and too tedious for a review, and proceed to the more agreeable duty of examining,

Chapter 8th and 9th, the subjects of which are, "Of the preventive treatment necessary to be adopted for the preservation of the health and beauty of the teeth and gums," &c. &c.

An attentive perusal of these chapters, would almost lead one to believe they were not written by the same person who wrote the preceding part of the work, for in these we observe but few of those over-strained efforts to magnify circumstances of a trivial nature, and to give them a degree of importance which no principles of physiology or pathology can ever sanction, and which no experience can justify. On the contrary, with few exceptions, we observe a course of reasoning founded upon correct views, that plainly evinces something more than a vague and superficial knowledge of the particular subjects of which they treat.

Were all the views which are embodied in this work, as correctly conceived and as well supported by facts and experience, as are those contained in the chapters under consideration, they would have doubly enhanced its value in our estimation; for they are mostly written in a plain comprehensive manner. The reader, however, would doubtless, be better satisfied with a specimen of the author's method of treating the subject. We therefore introduce the following chapter.

"CHAP. IX. *Of the treatment of the Teeth and Gums of Children at the time of the second Dentition.*

"A proper attention to the second dentition is, in some instances, nothing more than successive applications of proper preventive means for the preservation of the health of the first set of teeth. and a preparatory treatment for the future health of the permanent one.

"Such is the treatment to be observed with strong and healthy children. In these cases all that is required of the dental art is, first, to assist nature in its natural process. if it is too slow; secondly, to retard it if it is too active in its proceedings; and thirdly, to prevent all morbid influence of the temporary teeth upon those of the permanent set.

"The first object is acquired by removing those temporary teeth, whose roots are too slowly or irregularly absorbed by nature; even though they should be free from disease.

"The second object is attained by preserving all the contiguous parts of the remaining first teeth, and of the permanent teeth, which are already visible, in a perfectly healthy state; by which means every unhealthy and irregular action, and consequently too rapid a progres in some, and too slow a progress in others, are prevented. By the removal also, of all such temporary teeth as are painful, from the lining membrane being irritated by caries, and of those which have already lost their vitality; as also by strict attention to keep the mouth of the young patient in a state of perfect cleanliness.

"The third object will in some measure, be gained by the means already employed, and likewise by the extraction of such of the temporary set as are so diseased that their caries are brought into contact with the permanent teeth; particularly with their lateral surface, even though the former should be free from pain.

In addition to the above local treatment, an attention to the general constitution of the individual will be very beneficial; the enjoyment of a wholesome atmosphere will be found of the greatest importance to the future health of the young subject; and a nourishing diet, perfectly free from stimulating properties, cannot be too much recommended.

"From these premises it will be observed, that caries only in its complicated stage is a certain indication for extracting the temporary teeth; and that in my opinion, it would be

very injudicious to remove indiscriminately all such teeth as are carious. It would however, be equally improper to perform any operation for the cure of such temporary teeth; such as filing or scooping away the carious parts, or stopping cavities with metal, as is sometimes done by ignorant operators.

"However simple the above treatment may appear to any one who is unacquainted with the subject, it nevertheless requires considerable knowledge of the parts, as well as great judgment and experience. Of the truth of this statement, the effects of the malpractices, so frequently to be observed by the attentive practitioner when consulted in cases of children, afford abundant proofs.

"The above treatment however, is sufficient only when the young patient possesses a good constitution, and when the teeth of the second or permanent set are all sound and regularly situated. Cases of this description seldom however, come under the observation of the dentist, as they are generally left to nature.

"The children for whom the assistance of the dentist is most frequently sought, are those who are either in a delicate, or at least an imperfect constitutional health: where the state of not only the temporary teeth, but of the permanent also is to be considered; and where both are found diseased, the future health and regularity of the latter requires the greatest consideration of the surgeon.

"Irregularity of the teeth is one of the chief predisposing causes of their diseases, and never fails even in the most healthy constitution to destroy, sooner or later, the strongest and best set of teeth, unless properly attended to. It is thus not only a most powerful cause of destruction to the health and beauty of the teeth, but also to the regularity and pleasing symmetry of the features of the face; always producing though slowly and gradually, some irregularity, but not unfrequently the most surprising and disgusting appearance; such as distortion of the under jaw to one side, a great elongation of that jaw and the chin giving the face that grinning or ludicrous, and sometimes forbidding appearance, which becomes particularly evident and characteristic at some future period of life.

"Especial attention should be paid to these disorders at an early period of life, for the slightest irregularity in the teeth of the child may become a sufficient cause, not only of the above deformities, but also of the ultimate destruction of the teeth.

"Under such circumstances, the dental treatment of the permanent teeth becomes very complicated, as it is necessary, not only to take into due consideration the diseases and irregularities of the teeth already shed, but also to judge from the state of them of what we may expect from such of the permanent teeth as are still enclosed in the gums, many of which are sometimes found at their first appearance to be affected by various disorders or irregularities. We may form an idea of the judgment which is requisite for a proper treatment of such cases, and of the embarrassing situation of the inexperienced operator, by considering that every one of the permanent teeth requires six or seven years for its formation, before it perforates the gums; that nature requires fourteen or sixteen years for the formation of the permanent set, with the exception of the four wisdom teeth; and nearly thirty years including these; and lastly, how necessary it is to be well acquainted with the various influences, both healthy and morbid, to which the teeth are subjected during those several periods.

"It is however, a great pleasure to know, that dental surgery is abundantly provided with the most sure remedies; and that in the most delicate subjects, if placed under proper care at an early age, the greater portion of the teeth of the permanent set may invariably be preserved in perfect health and regularity, in common with their relative and contiguous parts.

"By an early and judicious application of that preventive treatment, which I have just described, to the temporary teeth, and of all the means for restoring health and order, under the principles stated in the seventh chapter, to those teeth of the permanent set which have perforated the gums; and by judicious preparatory measures for a proper surgical treatment of such diseased teeth as have not been shed, I have never failed, even in the most delicate child, to obtain such a set of healthy and beautiful teeth as might be preserved during the whole life.

"The preparatory measures consist chiefly in preventing a crowded state of the teeth, an object often to be obtained by extracting some of the permanent set at an early period, in order to give sufficient room for the rest; it is therefore, a matter of great importance to know which teeth should be extracted for this purpose.

"Those teeth which are most subject to disease, least important, and the removal of which would afford the most

relief to the whole set, are the proper ones to be chosen for extraction.

"As the loss of the incisors and cuspidati greatly disfigures the set, they ought to be always preserved if possible; and I have hardly ever seen a case in which it was necessary to extract any of them with the view to give room to the rest, where an early attention had been paid to the state of the teeth.

"The preservation of the bicuspides also should be a matter of particular consideration; and the usual practice of extracting the first bicuspid teeth to make room for the cuspidati, ought to be avoided, as well as the removal of the lateral incisors, by an early treatment.

"The first molares are generally most predisposed to disease: they are least important, as it regards both appearance and utility; and so situated as to afford by timely removal, sufficient room for the anterior teeth, as well as for the second and third molares. If these teeth are extracted at any period before the age of twelve years, all the anterior teeth will grow more or less backwards, and the second and third grinders so much towards the anterior part of the mouth, as to fill up almost entirely the vacant spaces caused by the removal of the first molares.

"In almost every instance, all irregularity will be prevented by this treatment, and all the teeth will take a proper situation. But besides this advantage, another more important benefit will invariably follow; viz. all the teeth will be improved in strength and health, and particularly the dentes sapientiæ, which will sometimes penetrate the gums much sooner, and prove of larger size and possessed of greater firmness than usual; a fact to be more particularly treated of hereafter, in that part of the article on caries, which relates to the causes of that disease in its simple state.

"I must here, particularly observe, that to obtain these desirable effects, it will be absolutely necessary that all the four molares should be extracted, and if possible, at the same time, or at least with very short intervals interposed.

"The partial removal of these teeth will, in most instances, not only be entirely useless, but even become the actual cause of such irregularities as are sometimes totally incurable at a later period of life.

"If for instance, the molar tooth be extracted on one side, and not on the other, all the remaining teeth will during their growth, bear an inclination to the side from which it has been removed; and if one or both of these teeth be extracted

from the upper, and not from the under jaw, considerable disproportion between the two jaws will be produced.

"I have very often observed, that the deformity, which consists in the shutting of the under incisors and cuspidati over the upper teeth, has been produced by the injudicious extraction of some of the teeth of the upper jaw, with a view to restore irregularities of comparatively very little importance, without taking proper care to secure a due proportion between the upper and the under jaws.

"Indeed, these permanent first grinders, the formation of which commences soon after birth, and which generally make their appearance at the age of six or seven years, are usually found more affected with caries when they first appear than any other teeth; and if in a delicate subject, even but one or two of them should be visibly diseased, some original defect may generally be suspected to exist in the others; and hence, the best treatment is the removal of all the four as soon as possible, in order to preserve the perfect health, as well as the regularity and beauty of the other teeth.

"When proper attention has been paid in the manner stated, to the shedding of the teeth; when the diseases of the permanent teeth have been properly treated; when a sufficient regularity or suitableness of situation has been obtained, in order to afford facility for the requisite operative treatment of such diseases as, must unavoidably arise from the original defects now supposed generally, indeed, existing, but which may become visible at a later period in any of the remaining permanent teeth; and lastly, when such caries is judiciously treated as soon as it appears, and followed by a constant application of a proper preventive treatment; a certain and permanent health of the parts is the natural and agreeable consequence."

We shall now proceed to notice the second part of the work. In this the author commences with:

"General remarks on caries of the teeth."—In which he observes the following: "I believe I may justly assert that this malady has hitherto not been properly understood, nor satisfactorily described by any surgical writer." (P. 207.)

From the tenour of this and a number of analogous passages in the work, many would be ready to say if these are to be relied on, very little indeed, must have been known of this branch of medical science. However this may be, we can scarcely refrain from saying, that if the author had until recently, never visited this nether world, and knew not that

a work had ever been written upon the subject, we should not have wondered at the latitude which he has given to his opinion in the present instance.

Whether the disease in question has hitherto been "properly understood," or "*satisfactorily* described," to every one or not, we are not prepared to say: But this much is certain: It has been repeatedly described by different writers, in all the forms and conditions, and under all the modifications in which it has ever been seen, or perhaps ever will be! and with a degree of accuracy that can scarcely be exceeded. And as to its having been "properly understood," we will just observe that, in the practice of medicine it is pretty generally believed that when a disease is susceptible of being successfully treated, in all its forms, it must be *pretty well* understood.

By this rule we must say that caries of the teeth is properly, if not *perfectly* understood, and that too by very many; for it has been and daily is successfully treated in its various forms and conditions; and moreover, the very nature of caries in the teeth is so far "properly understood," that we *know* that it can be, and is in numerous instances successfully checked in its progress by different topical applications, without any reference to a manual operation. This we believe is another proof that the *nature* of caries in the teeth is pretty, well understood. If, however, this should not be admitted, we are at a loss to determine by any thing which the author has advanced, what part or point of the subject is more clearly and satisfactorily elucidated than before? It is true that he has told us that "caries, in fact, is that state of the tooth in which *mortification* has taken place in one part, and inflammation in the part contiguous to it, the *former originally* produced by the latter, and the latter *kept up* by the continual contact with the former." (P. 211.)

Now as mortification implies either an active or passive inflammation, we are left to suppose that in *external* caries of the teeth, one or the other must have existed, and this too in a part, (viz.,) the enamel of the tooth, (upon the external *surface* of which it *always* commences,) which it is well known, possesses neither *sensibility* nor vascularity in the *least possible* degree.

If then inflammation in the enamel, (speaking of external caries,) produces inflammation in the contiguous parts, (viz.,) the bone, or, on the contrary, inflammation of the bone of the tooth occasion inflammation, and mortification in the enamel; we would seriously ask, what kind of inflammation is it that

can be excited or lighted up in a substance which, as before, is neither endowed with *vascularity* nor *sensibility?* To us, it is an enigma.

The fact is, we have the most satisfactory proofs that external caries, (which is by much the most prevalent and moreover dangerous kind,) commences upon the external surface of the enamel, at a small point or points, at which the enamel is gradually *decomposed* until it reaches the bone, when actual caries commences, and unless seasonably checked, goes on to the entire destruction of the tooth.

Now by way of elucidating this subject, we should have been much gratified had the author endeavoured to explain more satisfactorily, the *nature* and *cause* of this, yet, mysterious process.

It is true that of the causes of simple caries, he says "the local causes are diseases of the gums, periosteum, and maxillary bones." (P. 229.)

Now if diseases of the two latter, occasion decay of the teeth, why does not caries attack the parts the most contiguous; viz. the roots of the teeth which are immediately surrounded by the diseased maxillare bones and investing membrane or periosteum? If the former, or diseased gums occasion decay, we wish to know by what means, or in what way or manner they produce decay. The author speaks (at P. 229) of "various external *influences*, both *chemical* and mechanical," as agents in this business; but he no where, we believe, tells us what these *chemical* influences, or agents are —Had he explained this part of the subject, we have reason to believe that he would have given us a key by which the whole might easily be comprehended, and "properly understood." As it is, the general reader is left nearly as much in the dark, as at the beginning of the chapter. Nevertheless, the author has treated the subject with considerable ingenuity, and with some judicious remarks, and, moreover we are pleased with the frankness with which he speaks of Mr. Hunter's work on the teeth, and the principles which it inculcates.

We have frequently known instances, in which persons have entertained an opinion, that if they could obtain and read "Hunter on the Teeth," believing that no other person had written expressly on that subject, they would require no other shield of protection, or introduction to an enlightened community as dentists of the first order.

So far from its being calculated as such, we do not hesitate to say, that no work on the same subject abounds more with

gross absurdities and palpable contradictions, which, in part, is not so much to be wondered at, as he was by no means acquainted with the subject of which he wrote, with the exception of his views of the natural history of the teeth, and even in this, he has advanced some erroneous opinions.

On the subject "of surgical treatment of simple caries," and "of complicated caries," &c. &c., P. 234 & 36 the author appears to be familiar with that course of treatment which would, most likely, conduce to the preservation of the teeth; and his remarks in general, are justified by experience. The same observations will apply to his views "of the surgical treatment of complicated caries;" but, as it respects tooth-ache, P. 245, and "the morbid influence of dead teeth and roots upon the living teeth," &c. &c., we are disposed to believe that, with a few exceptions, he is wrong in his premises, which plainly evince a more familiar knowledge of the necessary local treatment in those cases, than with the phenomena of the nervous system, and also of inflammation, when it attacks the organization of the dental apparatus.

We think however, that what we have already observed, on the subject of teeth deprived of their vitality is a sufficient commentary upon this head, and shall therefore wave any further remarks.

The next subject which presents itself for consideration is that of the devastation, or absorption of the gums and sockets of the teeth." Chapter 3d, P. 271.

The substance of this chapter appears to be nearly the same as that of an essay written by the author, and published in the Philadelphia Journal, and which was freely commented upon, in a paper published in the Medical Recorder, volume 5, p. 23, to which we beg leave to refer the reader.

Chapter 4th—Treats "on the operation of extracting teeth," &c. &c.

Upon a subject so long, so generally, and so well known, many, perhaps, would be induced to believe that but little or nothing could be said that was in any degree calculated to obviate the difficulties, alleviate the sufferings, and promote a more generally safe, easy, and successful result in this, truly unenviable, operation; but this is an erroneous opinion. For notwithstanding it has been so long and so generally known, and, we are sorry to say, must necessarily continue to be so, yet it is one which, notwithstanding the many ingenious attempts, and, in some instances, successful improvements that have been made in instruments for this purpose, in numerous instances it has been, and is still attended with the most

serious consequences; and that too from the inexcusable want of a correct knowledge of the parts, and of the principles and powers of the instruments exercised upon those parts, by those who employ them.

We are aware that almost every dentist of *experience* and judgment, has become so familiarized to the use of certain kinds of instruments which he makes use of in this operation; and to certain methods, which he varies according to circumstances, that, in almost all cases, he is sure of a safe and successful result, and therefore stands in need of but little instruction in this operation. Nevertheless, we are pleased to see the subject taken up by one who is, apparently, so competent, and whose *unsparing* pains to provide instruments suited to every possible case, and whose *experience* has rendered him so *familiar with the operation*, in all the variety of cases that occur in an extensive practice.

From such a source we may reasonably hope for information, and for some valuable hints. In this we are not disappointed—For, although, we are not disposed to yield our implicit assent to all the author's opinions and views; yet we are ready to acknowledge that this part of the subject, contains, in general, many judicious remarks, and much useful instruction; we are more especially pleased with the author's remarks on the impropriety of resorting to coersion and to deception in all "dental operations," whether upon children or adults, and we hope that his opinions on these points will meet with general approbation.

Chapter 6th.—"Of the operation of stopping carious teeth," P. 381.

Amongst the various operations allotted to the profession of a dentist, there is no one, however simple it may appear, of more importance, or better calculated to render essential and lasting benefits, when judiciously performed, than that of stopping, or plugging decayed teeth with gold or some other suitable metal.

In this nice and often difficult operation, we are ready to attest that the author, in some of the *latter* years of his practice in America, had acquired a degree of skill that we could wish, from motives of humanity, was more generally and better known by those whose duty it is to practise it.

Hence we might reasonably expect some useful hints, and much valuable instruction on this head. In this too, we are not disappointed; on this subject, the author has given some "practical remarks on the operation," on "the present imperfect state of the operation," on instruments used for that

purpose, "on improper materials used for stopping teeth;" of "certain injurious methods of stopping teeth," &c., in which is embraced a variety of circumstances of great practical utility and importance, though but little known, if ever thought of by many dentists of this country, who consider themselves too well informed, and too wise ever to be taught, or to need instruction upon a point, or points, so simple in their nature, and apparently, so easily performed.

In the seventh and last chapter, though not, by any means the least in importance, the author speaks "of the surgical treatment of the lining membrane, or nerve of the teeth, when exposed; previous to the stopping of the cavity with gold." P. 423.

This chapter as it relates to the treatment of carious teeth, for their preservation, may be considered as a continuation of, or part of the preceding chapter, and of equal if not more importance, as it points out the necessary course to be pursued preparatory to the stopping of teeth with gold, or other metals, and which, when requisite, is indespensably necessary to a successful result.

In these two chapters, the author has evinced a degree of judgment and skill, in this part of his profession, which if acquired by his own practice and experience, would justly entitle him to more than a common share of merit; but this appears to us in some degree doubtful. For, although the author, in the exposition of his views, has served up the subject in a different style, and is far more copious than some of his predecessors, though not however, without advantage, yet his views and treatment are, by no means new, at least as it respects the principal points, for in that country where "abuses and malpractices of dental surgery, are quite the order of the present day," the same maxims have been practised and inculcated for nearly or quite one hundred years. We would readily give the quotations, had not the subject already led us beyond our limits. We will however, refer the reader to the works of Tauchard, volume 2d, from page 53 to 84 inclusive; to the works of Courtois, from page 65 to 72 inclusive; and to the writings of Geraudly, from page 46 to 49.

It remains to make a few observations on the general character of the work before us.

With the principal scope of the author's views, no person will remain long unacquainted, if he will bestow a common share of attention in the perusal of the work.

The author's manner and style is often easy, free and com-

prehensive, in many instances however, it is extremely verbose. The most discordant feature observable in the work, however, and which very few will fail to notice, is the too frequent repetition of technical and professional phrases, which abound, more especially, in part the first.

This we have been led to observe more particularly, inasmuch as the author, in his preface makes the following remarks, "and that he might not fail to make himself intelligible, he has *scrupulously* avoided technical terms, as well as every thing that might have the appearance of scientific mystery." P. ix.

Now, without enumerating a number of instances that occur, it will be observed that the terms, "Dental Surgeon and Surgery," occur about sixty times; and mostly in part first. In some instances, it is repeated three times in a single page.

The terms dental surgeon and surgery, dental maladies, dental diseases, dental art, dental branches, dental operation, dental nerves, dental literature, dental practice, dental bones, dental treatment, dental assistance, &c., occur about one hundred times in the work.

It is not the propriety or correctness of the terms that is called in question in this instance, as they have mostly, been long used by the French writers, agreeable to the idiom of their language; but it is the necessity or propriety of the repeated application of the adjective dental, to nerves, maladies, diseases, treatment, operation, &c., when they relate to the teeth, because it is the subject of which he is treating, and therefore the terms, when used, are not likely to be misunderstood.

Much useful information, however, may be obtained from the work; and from this consideration alone it may be read with advantage to the physician, and especially to the dentist.

UTILIA FIDELITER EXCERPENDUM.

ANALYSIS

OF

AMERICAN MEDICAL JOURNALS.

ART. I. *North American Medical and Surgical Journal, for October*, 1826.

A PAPER written by P. Merrill, M. D. on the epidemic of 1825, in Natchez, first presents itself to our notice.

Dr. MERRILL, in the essay before us, says: That "the epidemic which is the subject of his remarks has exhibited *conclusive* evidence, of the *general* origin of its causes." (Page 218.) On the next page he states, that the occurrence of the disease, under the hill in Natchez, "appeared *evidently* to depend for *its causation*, upon the *single* circumstance of the construction of a *wharf*;" and on the hill in Natchez, "to extensive accumulations of *loose* earth collected by an *effort* to graduate the uneven surface of the city." Although Dr. Merrill pretended to trace the origin of the disease to local causes, viz: a *wharf*, and *heaps* of loose earth, yet we find him afterwards denying his own doctrine; for he says, "All inquiry into the existence of *local* causes seems to be *wholly* unsatisfactory." Thus, according to Dr. M., the yellow fever of Natchez arose from *general* causes in *one* page of his essay, and from *local* causes in *another*. There is no way of reconciling this contradiction, but to admit that those *heaps of loose earth* in Natchez, are general causes of yellow fever. Dr. Merrill seems to think so, for he states that the diseases of the country "not unfrequently terminate in black vomit." If this be the case, what mischief have those pernicious accumulations of loose earth in Natchez done to the world, by producing the yellow fever in the country, among the planters, in New Orleans, the West Indies, and other places! Speaking of the

general origin of yellow fever, the author begins and ends a full sentence thus: "Such is generally the case in Havanna and the other West India cities, New Orleans, &c. and also in Natchez, with the epidemics that occurred in the years 1817, 1819, and 1823, *supposed to have been caused* by exhalations from extensive accumulations of *loose* earth collected by an effort to graduate the uneven surface of *the* city." It is evident that the gentleman means that the accumulations of loose earth existed in Natchez *only*; for no mention is made, nor the smallest hint given, of extensive accumulations of loose earth in the other places mentioned. The necessary inference is, that New Orleans and the West Indies, have suffered alike with Natchez, from this general cause of disease.

If this gentleman's doctrine be correct, it behoves the friends of humanity, to remove *at all hazards*, those "extensive accumulations," or *heaps* of loose earth in Natchez, *(if any such exist,)* which being general causes of disease, exert such a pernicious influence over Natchez, the surrounding country, New Orleans, and even the West Indies. It is but justice, however, to Dr. Merrill, to state that he falls out with this *loose theory, loose earth and all;*—For although he had endeavoured to make us believe that the yellow fever of Natchez in 1817, 1819, and 1823, was produced by accumulations of *loose earth*, and in 1825, by *loose* earth and a *wharf;* yet a few pages further on, he advocates the following doctrine, viz: "The whole history of the epidemic of 1825, when taken in connexion with those of 1817, 1819, and 1823 affords us," says Dr. Merrill, "little reason to doubt, that the causes which have produced the *several* visitations *upon this city*, were of a *general* nature, producing their effects upon its whole extent simultaneously, and that *these* have mainly depended for *their* origin upon the *occurrence* of much *wet* weather during the winter and *spring* months, followed by unusually *dry* and *hot* weather in August and the *autumn* months." (P. 221.) This last notion concerning the origin of yellow fever, Dr Merrill, probably, introduced to accommodate some of his friends, (perhaps the person who furnished him with a *table of the weather;*) for we cannot think that Dr. Merrill actually believes in this *wet and dry weather theory*, although *he gives it as his own*, because he has laboured too hard to establish a different doctrine, viz. the *loose earth and wharf system.* His labours on this head we are now about to investigate. Previously to which, it will be well to mention that in as much as Dr. Merrill has stated in his essay, that the yellow fever of 1825 under the hill in Natchez, was produced by *a wharf*, built in the *winter* and *spring of* 1825, that houses were erected on this wharf, and that the yellow fever was principally, if not entirely, confined to those houses *upon* the wharf; and in as much as we had published an essay in our 34th No., purporting to be a *faithful history* of the same epidemic of which Dr. Merrill treats, and there being no mention made by our correspondent of the existence of the epidemic *upon* a wharf built in 1825, and in the houses erected on the wharf, we thought it a great piece of neglect in the author of the essay, not to have mentioned *something* about these new houses, new wharf and the yellow fever which existed *upon* it,—(that is to say, if any such wharf or houses were *actually built in* 1825, *before the occurrence of the yellow fever.*)

In order that we might be enabled to substantiate the essays published in the Recorder, it was thought advisable to send to Natchez, for information.

The interest of the Journal demanded that we should take this course, the cause of truth and science required it, and the obligations we were under to the profession, made it a duty incumbent on us. We published what purported to be a *faithful* history of the apparent sources of the yellow fever of Natchez—another Journal, six months afterwards, publishes an essay which endeavours to show that the history of the yellow fever as published in this journal, was *not a faithful* statement; that a yellow fever, which existed in Natchez *upon* a wharf, *built in* 1825, in store houses erected upon the said wharf,

was *wholly* omitted by our correspondent, and who at the same time wished to *impose* his account of the disease upon us, and the world, as a *faithful* history. Further, our correspondent stated, that "the disease Under the Hill, in Natchez, in 1825, continued to prevail throughout September and October." The author of the essay under review, has twice denied this assertion of our correspondent, in *unqualified* terms. Let us enter into the store houses, which, Dr. Merrill says, were erected on a wharf, *built* in 1825, and see the poor souls, labouring under the yellow fever. Reader tread lightly! for we are now about to enter where—"tenues sine corpore vitas—volitare cavæ sub imagine formæ;" but first let us listen to Dr. Merrill, and hear the history of the yellow fever under the hill in Natchez: "In the winter and spring of 1825, the first wharf for the landing of steam-boats, was constructed Under the Hill, and some store houses having been erected *upon* it, before it was completely filled in with earth, considerable vacancies were left under them, which afterwards, became receptacles of rain water and the washings of the street above. The whole wharf, some hundred feet in extent, and six or eight in depth, was washed on one side by the river, which rose during the spring months, nearly to its surface, and, together with the constant rains, kept the immense mass of loose earth of which it was constructed, for a long time filled with water," (P. 218 and 219.) This wharf built in 1825, which has done so *much mischief*, had *no existence at the time* the yellow fever occurred in Natchez, but in the *imagination* of Dr. Merrill, the extensive accumulations of *loose earth* were not *then* "collected by an *effort*." (For the *first* effort made to collect them, took place in November, 1825, after the yellow fever had disappeared.) The houses which Dr. Merrill has placed on the wharf, built in 1825, are not built of wood, stone, brick, or any other *material* substances; the inhabitants with which he has peopled them, have neither flesh nor blood, but both the houses, the people and the wharf, are creatures of Dr. Merrill's *fancy*, created, it would seem, *pro tem*, for the especial purpose, of establishing *his loose earth theory*. Lest the reader should doubt what we have said, we will here introduce the certificate of the harbor master of the city of Natchez.

I hereby certify, that the *only* wharf, which was built Under the Hill, in Natchez, in the year 1825, was not commenced before the month of November, of 1825. There was a wharf built in 1824, and but two storehouses have been erected upon it—and, but one storehouse has been erected on the wharf built in 1825; and this, was not finished until 1826, and no person has ever lived in it—it being used as a storehouse for cotton, &c. *There was no yellow fever on the said wharfs in* 1825 *or since.*

SAMUEL W. BUTLER,
Harbour Master, for the city of Natchez, for 1824 and 1825.
November 19th, 1826.

Hence it appears that "the temporary existence of the disease upon the newly made wharf," is nothing more than a temporary attempt to support a temporary theory, to abrogate the facts contained in the Medical Recorder, and to substitute in their stead the closet speculations of a hypothetical mind. If Dr. Merrill has come within a year of the fact, and alludes to the wharf built in 1824, we have but to say, that *within* that year, the wharf must have been pretty well *pounded* by hogsheads, barrels, bales, carts, drays, horses and the men engaged in freighting the steamboats; and so his *loose earth theory* would not hold good, even had the yellow fever, as he says, been confined "*upon* the wharf." But unfortunately for his theory there were but two houses upon the wharf, and the harbour master we are informed occupied one of those houses for an *office*, and he expressly states that "there was no yellow fever upon the wharf." If Dr. Merrill alludes, as he says he does,

"to a wharf built in the winter and spring of 1825," then all we have to say is, that this wharf, its inhabitants, "the houses erected upon it," and "the vacancies left under the houses have disappeared," "*loose earth, and all* before the light of truth and,

> "Are melted into air, into thin air,
> And like the baseless fabric of a vision,
> Left not a wreck behind."—

In the essay published in our Journal it was stated "that the disease" under the Hill in 1825, "continued throughout September and October." But Dr. Merrill in his essay says, "the existence of the disease at this place, where the first cases occurred was of short duration, and did not spread, but *wholly* disappeared on the desertion of the wharf-district, which took place immediately, on the alarm being given."-219. The alarm he says, was given August 29th. On the next page he again tells us, that, "the infected district confined to a very small square consisting altogether of store houses, was soon abandoned by all its inhabitants, which terminated the progress of the disease at the city landing for the season." Although we do not doubt the correctness of the statements made by Dr. Cartwright, we deem it advisable to publish the following letter addressed to him by Dr. Denny, a physician of Natchez, and health officer of that city.

Natchez, Nov. 7, 1826.

Dr. S. A. Cartwright.—Dear Sir. In answer to your inquiry of yesterday, respecting the prevalence of the yellow fever *under the hill*, after the general alarm in 1825, I beg leave to state that I attended *cases* of that disease *under the hill*, as late as the middle of October. Mr. Harrington, who died on the 21st of October, and James Camden, who died on the 16th, you will recollect, *you* visited them with me. About the 24th I visited a case of this disease at Mr. Broadwell's* with Dr. Chew. I had a patient labouring under the same disease at the same place and period whose name I have not ascertained.

Respectfully yours, &c.
J. A. Denny.

If corroborative testimony be necessary, we subjoin a certificate from the sexton who interred the dead.

I hereby certify, that I buried in the grave-yard near Natchez, twenty-two persons from under the hill in Natchez, who died during the epidemic of 1825. Of these 22 persons—5 died in August—9 in September, and 8 in October, 1825. Of the above deaths, a majority in *each* month were reported malignant or yellow fever.

(Signed) Robert Stewart, Sexton.

27th Nov. 1826.

We ask, has not Dr. Merrill mistaken the road to fame? To ascend the rugged hill of Medical Science, "from whose summit Fame's proud temple shines afar," it is necessary to beware of *loose assertions*, for, like *loose earth*, they are too *apt to give way*. Dr. Merrill goes on to state that "the disease, with the exception of its temporary existence upon the newly made wharf, has *never* been traced to any particular local origin," but says "it has in *every instance* been *general* in its origin," 221. Now if he will read the account given in the Recorder, of the yellow fever of Natchez, in 1823, and in Natchez and Washington in 1825, and Dr. Gustine's statement of the origin of the yellow fever of 1817, he will find that the disease has in every instance,

* Lives under the Hill.

been traced to *local* causes, and has in every instance been local in its origin; viz. in 1823, from the 7th to the 20th of August, it was confined in and near *one* house which contained a large quantity of putrid bacon, beef, and fish, and yet Dr. Merrill asserts "that no new and unusual accumulations of either animal or vegetable materials in a state of decomposition have been discovered, in sufficient abundance to account for a local origin of the disease." In 1825, the yellow fever under the hill first occurred in a ware-house (but not on a wharf.) The ware-house contained a quantity of putrescent beef or pork, and spoiled porter. The owner of the ware-house we have understood was inspector of the former article, and of course his ware-house contained a large supply. He was constantly engaged in repacking, and permitted the brine to run through the floor into a reservoir, which had no outlet, there to putrify with the spoiled porter. About two weeks afterwards the yellow fever occurred in Washington, after which it broke out in three different places in Natchez, on the hill, also about a mile in the country. In all which places it was local in its origin, and has been traced to local causes. Yet Dr. Merrill must boldly put his *ipse dixet* without proof or support, against a mass of facts collected seemingly with great care and caution by Dr. Cartwright, and published in this journal—facts which are supported by evidences which Dr. Merrill, we think, never can disprove, even should he be so fortunate as to prove that the yellow fever was produced by a wharf, before the wharf was made, and that people died in houses that have never been built. The only way to improve the science of medicine is by facts, observations, and fair inductions. One of the principal objects of medical journals, is to open to physicians a repository, where each and every physician, throughout this republic, who feels so disposed, can bring forward, without expense or trouble, the fruits of his experience. We are publishing a series of essays from the pen of a physician in the south. Dr. Merrill's principal object in his essay (for he has added nothing new or useful) seems to be an attempt by direct *assertion and insinuation* to prostrate, at the shrine of his speculations, a large share of those facts which our correspondent has been at the trouble, to collect and arrange. Dr. M. has been labouring therefore, *not* to improve the science of medicine by a collection of facts, but to *scatter* those which have been collected, that refuse to bend in obedience to his hypothesis. He seems to have had another object in view, viz. to represent *himself* as head of the profession at Natchez, the only *wise* and *skilful physician*, and all the rest as scarcely meriting even the appellation of *quacks*. For Dr. M. would lead us to believe that the *planters* in the vicinity of Natchez know *better* how to treat diseases than all the doctors *put together, himself excepted.* Yet who would believe that among the number Dr. M. puts a black-mark on, *en masse*, could be enumerated, besides Dr. Cartwright, the author of the essays in the Medical Recorder, the names of Bell, M'Pheeters, Chew, Hunt, Walker, Gustine, Denny, Provan, Perrine, &c. &c. physicians, all of whom are highly respectable.

Dr. Merrill's essay, so far from being a history of facts, seems to us to be nothing more than a history of the *vagaries* of the author's own mind, and as such we would beg leave to ask the question,—is not the said essay an *imposition* upon the medical profession? We would advise Dr. Merrill to find some better employment, than that of vain attempts to *undermine* the *substantial* labours of others. The *labours* of men who devote their time and talents, to the improvement of their profession, regarding neither their health nor pecuniary concerns, and having nothing in view, but public good, the interest of science and the discovery of truth, and wishing no other reward, than an honest fame, the pleasing thought that they have not lived in vain; we say the *labours* of such men are not so *easily destroyed* as Dr. Merrill may suppose. Dr. Merrill may array the phantoms of his own mind against matter of fact, but the latter will stand, when the former have vanished before the light of truth. He may rush and *work against* facts, as much as he

chooses, yet like pure metal, they will shine brighter and brighter. He may, like a troublesome *fly*, *buz* for a time, about the faces of those who are labouring in the fields of science, but the *brush*, not *fame* awaits him.—But it is time that we take a view of the symptoms and treatment of the disease, as contained in the essay under review. Dr. Merrill states, that "Nearly every case was strongly marked, by appearances of inflammations or *congestions*, either *general* or *local*. (P. 222.) He again states that "*cases of congestion either general or local*, were commonly the succeeding symptoms of inflammatory attacks." (P. 223.) For a physician to talk about *a general congestion being a symptom of disease*, shows not only an ignorance of his profession, but an ignorance of the *meaning* and *etymology* of words. We can scarcely form a conception of a general inflammation, and would doubt its existence *even in the case of Job himself;* but a general congestion has no parallel in history. This general congestion of Dr. M. must be *terrifying* beyond measure, for he says that the cases of it, "are attended with the *most excruciating pains*," and it is by these *most excruciating pains* that he *distinguishes* them from the inflammatory cases. We all know that inflammation is painful enough in all conscience, but if this *general congestion be more so*, God save us from it! The congestions we have seen and read about, have always been *less* painful than inflammation. But it seems that this general congestion of Dr. Merrill, has escaped the notice of all others but *himself*. It is a wonder that those *most excruciating* pains, which attend it, had not caused it to be noticed sooner, especially as he tells us, it is attended with, "a pearly whiteness of the sclerotica of *the eye*." We expect, that when Dr. M. put down this last symptom, he forgot he was writing on yellow fever, for he directly afterwards recollects himself and says, "The eyes in the latter stage, were *suffused with blood*." Now, if these congestions succeeded inflammation, as he says they commonly did, they must have occurred *in the latter stage*, and thus the eyes, as the gentleman says, must *truly "have exhibited a turbid and unnatural aspect,"* viz: *red* and *white at the same time*. (P. 223.) But let us proceed to the treatment. Dr. Merrill says, that "in *every* violent attack of this disease, the first remedy *invariably* applied was blood-letting." Would the reader wish to know, what Dr. Merrill next *invariably* did? He *invariably* purged and now and then gave a *glyster*. This constitutes, the whole of his practice. But Dr. Merrill speaks of a remarkable *error* among physicians (the physicians of Natchez, of course,) viz: "The *necessity* of so varying the purgative treatment, as will certainly *prevent* the occurrence of *frequent watery discharges*." He proceeds, "No prescription is now more common, than that of giving 15 or 40 drops of laudanum, after the doses of calomel have produced a certain number of discharges, or when these have assumed a *watery* appearance; and I do most conscientiously believe, that no practice was ever more *destructive*." (P. 229.) This is another indirect attempt to impeach the facts contained in the Recorder. Two pages further on, Dr. Merrill states that "*Watery dejections, rapidly debilitate the patient;*" yet he *denounces* the most effectual means, to *restrain* them, as most *destructive*. To prove the baneful influence of opium in epidemic fevers (*as if all epidemic fevers were alike,*) Dr. Rush is referred to, and Dr. M. says that "this remark ('the baneful influence of opium in epidemic fevers,') will be found *true*, by all who follow the footsteps of Rush, in *impartial* inquiry." Let us see *how true* it will be found. In his account of the yellow fever, of 1798, Dr. Rush states that "Laudanum was acceptable and useful in many cases of pain, wakefulness, *vomiting* and *diarrhœa*, after the use of depleting remedies. (Page 48, vol. iv, ed. 4th.) In this fever, Dr. Rush found the bark serviceable, he also used *emetics*. In the yellow fever of 1794 and 1797, Dr Rush likewise used *laudanum*. "As well," says Dr. Rush, "might a traveller describe a foreign climate, by the state of the weather, or by the productions of the earth, during a *single* autumn, as a physician adopt a uniform *opinion* of the history and *treatment* of a fever, from its phenomena in

any *one* country, or during a *single* season." (Page 50, vol. iv, ed. 4th.) Yet Dr. Merrill wishes to support, by the authority of Rush, the *sweeping* assertion "of the baneful influence of opium in *epidemic fevers*." Dr. Merrill continues, "I have for more than two years PROSCRIBED this article (opium) *entirely* from my list of remedies, for the autumnal fevers of this climate, and during that period have *not given one grain in such cases*." (P. 230.) To this we have only to say that he who *proscribes entirely*, any article of well established reputation, evidently shows that he has *improperly used it, or is ignorant of its qualities*. Such medical *bigotry*, stands opposed to the best interests of science.—Another practice which Dr. M. considers "*very reprehensible*," "is that of giving emetics *indiscriminately*." If he means that the physicians of Natchez pursue this practice, we will venture to answer for them that *no such indiscriminate practice is pursued*, (and we expect Dr. M. would hardly trouble the profession with a *critique* upon the practice of the Mississippi planters.) Emetics are used in Natchez, but from the best of our information, *not indiscriminately*. Emetics are recommended in the essays published in this Journal, on the yellow fever of Natchez. But they are only recommended in a *particular* stage of the disease: the stage is *described*. Nor are emetics recommended in *every* stage of the disease, *nor in any yellow fever, in which the stage described, does not occur*. Dr. Merrill is for substituting "warm water and chamomile tea," for tartar emetic and ipecac. in the treatment of *bilious fevers*. But does he suppose, that the physicians of the United States, who are so conversant with bilious fevers, will believe the assertion, that the *tea kettle* "will fulfil the indication of cure, with as much efficacy and more safety," than emetics? It is true that emetics are not useful in *all* bilious fevers, yet experience has proved their utility, in certain stages and forms of these diseases, and proclaims they have no substitute. But, to put emetics out of the question, a case is related (P. 232.) of a soldier (alas poor fellow!) who being, "attacked with *all* the symptoms of *a violent fever*," took *repeated* doses, of a solution of epsom salts and tartar emetic, in an infusion of senna. This medicine we are told produced not only a vomiting, "but also a purging of numerous limpid stools," and "no medicine could be retained upon the stomach, a sufficient length of time to produce a *favourable catharsis*." The vomiting, we are told, "terminated in black vomit,"—a sure proof that it was produced by the tartar emetic, contained in the senna and salts. Now, if the reader will examine Dr. Merrill's essay, at page 222, and page 227, he will find that "constant nausea and vomiting, hiccough, &c." are enumerated among the "most common consequences" of the disease, and that "the affection of the stomach," "this peculiar location of the morbid affection," "gave rise to the most distressing and fatal symptoms." "The stomach was *usually found* affected with a constant nausea and disposition to vomit, and the bowels in nearly *all* cases *suffered from obstinate constipation*." But in the case of the soldier, we are told, that numerous limpid stools were produced, and no medicine after that above mentioned, could be retained on the stomach a sufficient time, to produce a favourable *catharsis*." Is it not evident from Dr. M's own account of the matter, that the vomiting was the *natural effect* of the disease, and the *watery purging, the effect of the medicine?* Query, would not a *little opium, or an injection of laudanum* to check the watery discharges, which Dr. M. himself says, "*rapidly debilitate the patient*," have been highly serviceable in the case of the soldier? But unfortunately, we fear for the *poor soldier*, opium was a *proscribed* article. His physician had not given "one grain of it," he says "in two years in autumnal fevers." The results of this absolute exclusion of opium," he tells us, "has been *satisfactory beyond his most sanguine anticipations*." We see the results of this practice, in the case of the soldier, it is just such as we would have *anticipated*, but to us, at least, such a result is by no means "*satisfactory*."

In corroboration of the general soundness of the views, statements, and practice of Dr. Cartwright, we shall, in conclusion, present to our readers some brief quotations from an account of the yellow fever as it appeared in the Queen's Regiment, in Barbadoes, in 1816 and 1817, by Alexander J. Ralph, M. D., assistant surgeon to the regiment, published in the Transactions of the Medico-Chirurgical Society of Edinburgh, vol. 2,—1826. In treating of the origin of the epidemic, Dr R. attributes it to torrents of rain that fell in September, which was tenaciously retained by the soil; to increased temperature of the atmosphere; and to a cessation of the trade winds, in consequence of which, he observes, "we had ceased to breathe an atmosphere purified by cool and wholesome winds, but now lived in one contaminated by the emanations of a soil that had become swampy." "The parade ground, a plain of about one mile in circumference, had numerous excavations in its surface, and at the time was badly drained." The disease first broke out in the huts* erected in the rear of the barracks, for the accommodation of the married soldiers, and which he considers as the *fons mali*. In continuation he observes, "Farther, it appeared that the disease was confined to particular *local situations* favourable to the production of noxious exhalations." In detailing symptoms he remarks, "The urine is pale and often not secreted during the whole course of the fever;" an observation which it will be remembered was also made by Dr. Cartwright.

In his notice of the dissections, he "regrets very much that their inquiries did not lead them to an examination of the spinal canal. The violent pain of back, which formed so striking a feature in the fever, depended, in all probability, on some inflammatory affection in this part." We regret it also, knowing, as we do, the valuable discovery respecting the diseased state of the ganglions and nerves derived from the spinal marrow, made by Dr. Cartwright, while engaged in such investigations, and which we have no doubt would have been confirmed by Dr. Ralph, had he pursued his dissections thus far.

Under the head of Treatment, he observes, "In the state of general depression mentioned under the second form, as marking the onset of the disease in some instances, where the patient lay languid and faint, his stomach very irritable, his pulse scarcely perceptible, his feet cold, and a clammy perspiration resting on his forehead, neck, and breast, I have witnessed the most beneficial effects from the administration of a full dose of laudanum, with warm brandy and water, &c." After noticing the manner in which calomel and jalap were used, he states, that "salts or salts with tartar emetic, would expedite and secure the effectual operation of the pills;" again, "after the bowels had been cleared, when the stomach continued irritable, pills, containing small doses of calomel, with or without opium, as three grains of the former with a quarter of a grain of the latter, and frequently with two grains of capsicum, were in general use; and often did every thing, tranquillizing the stomach, and preserving the bowels free and active." He, however, observes, "I never gave an emetic, nor do I think them to be at all admissible in this fever." One more quotation and we have done. "I was fond," he says, "in excessive irritability of stomach, of giving, after the first three or four days, a scruple of calomel, and ten grains of Dover's powder, at bed time. Three or four copious stools, sound sleep, and free perspiration, I have known to succeed the administration of this remedy."

We shall conclude our notice of this paper, by observing, that in those

* The huts were low, ill-ventilated buildings, constructed with a single deal board, and roofed with shingle. Each was commonly occupied by two families, composed of six or more individuals.

points where Drs. Merrill and Cartwright are at variance in their opinions, our readers will have no difficulty in making up their minds in favour of the latter.

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Drs. Bell and Mitchell, give an account of the natural and modified small-pox, as it occurred in Philadelphia in 1823-4, for some interesting papers on this subject. See Medical Recorder vol. 5th, page 224—257—268—378, &c.

Dr. Jackson, who is evidently an ardent disciple of Brousais; gives some very interesting cases of nervous irritation, exhibiting the efficacy of cold as a remedy; we select the following case. A young gentleman two hours after drinking a pint of undiluted gin, complained of great distress about the stomach and head, and shortly after convulsions supervened. Warm water produced vomiting, without any abatement of the symptoms; laudanum and ether occasioned an increase of distress and aggravated the convulsions; the face was flushed—eyes fiery, dilatation of the pupils—agonized expression of countenance—no coma—consiousness apparent; hot skin; pulse large, full and labouring.

Dr J. conceiving this state of things to depend on gastric, vascular and nervous irritation, gave a tumbler of cold water, sweetened—an abatement of the symptoms was the consequence, a second and third together with cold affusion were productive of a cessation of the spasms, venesection ad ℥xv. He was put to bed, but in the attempt to get there, the convulsions returned—affusion to the head, and another tumbler of cold water, arrested them; after which he walked up stairs—cold drink continued through the night; no renewal of convulsions.

Dr. Wood offers some novel views on the pathology of jaundice; he does not believe it to arise, in the majority of cases, from an obstruction in the liver, or its ducts, as is commonly supposed; but on the contrary inclines to the opinion that it depends on a deficient or suspended secretion of bile, and that the appearance like bile in the blood, urine, &c., is due not to the presence of bile itself, but to a proximate principle constituting the colouring matter of the bile which exists naturally and uniformly in the blood, and which it is the office of the liver to separate from it in the form of bile, if this be suspended it becomes redundant—the skin and kidneys take on a vicarious function, in order to relieve the system from this deleterious accumulation, and thence arises the symptoms of Icterus.

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Dr. Coates, gives an account of the origin of Dr. Physic's operation for artificial anus, in 1809, and proves the superiority of his method over that of Dupuytren, who uses the *enterotome*. Dr. P's operation consists in dividing the membranous partition which separates the two portions of intestine at the external orifice, and interrupts the passage of fæces to the lower portion. A ligature is passed through this partition, and secured by a slip-knot, so as to insure the adhesion of the two portions of intestine that constitute it, and prevent the possibility of opening into the peritoneal cavity. After a sufficient time has elapsed to produce this effect, the portion included in the noose of the ligature is divided by a bistoury; in the case related, fæces passed shortly after by the rectum.

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Dr. Cox relates a case of Asphyxia from drowning, together with some experiments illustrating its phenomena; we cannot give the details.

ART. II. *The Philadelphia Journal of the Medical and Physical Sciences, Nov.*, 1826.

ALEXANDER JONES, M. D., offers some interesting observations on the influenza or epidemic catarrh, as it prevailed in Georgia, during the winter and spring of 1826. The details are however not of such a nature as to admit of their being introduced here.

E. GEDDINGS, M. D., of Charleston, in a "note on the retention of the testicles" in adults, cites several examples of it in one or both sides, from various authors, together with their opinions respecting its supposed tendency to produce impotence, instead of which the mass of testimony goes to prove that such individuals are as competent, and are more lecherous than ordinary individuals. Persons of this description, have frequently been married, and the result was a numerous offspring, Mr. G. relates a case that occured to himself and then remarks, "from what has been said, it will appear manifest that the most serious errors might be committed in giving juridical evidence, when the case involved a question of sexual competence or rape."

Large doses of sulphate of quinine in fevers.—Henry Perrine, M. D., relates some cases of intermittent and remittent fever, where he employed the above article in a dose of eight grains in the first case, five grains every half hour in the second, in the third 16 grains in two hours, &c., in the fifth 8 grs. every third hour until sixty-four grains were taken, this latter case as it is a highly interesting one, we shall give in Dr. P's own words, "Mrs. P. a married female, apparently thirty years old, who had suffered from abortions, fevers, visceral inflammations, and was then labouring under chronic hepatic disorder; whose person is slender, deformed, and of originally weakly constitution, and whose sucking infant was her only living child, was attacked on Tuesday evening, 10th November, with a chill and fever, which lasted several hours. The next day at noon she was again attacked, and as the fever *continued* on Thursday, I was called to see her about eleven o'clock. She was insensible, except for a few minutes at a time, when roused by loud and repeated calls in an imperative tone, and she would then immediately sink again into a state of stupor. Her pulse was between 120–30; her veins were collapsed, and could not be filled by a ligature; her skin was dark and dry, and its temperature was increased on the trunk, but diminished on the extremities. At noon, a dose of eight grains of sulphate of quinine was administered, and an equal dose was repeated every third hour, until sixty-four grains were taken. At one o'clock a dose of purgative pills, (without calomel,) was given, and repeated at the end of six hours.

In fifteen or twenty minutes after the first dose of quinine, a *free* and *general perspiration* occurred, the extremeties becoming warmer and the body cooler; and the same circumstances were freshly manifested after each succeeding dose, but gradually becoming less remarkable. By bed-time she recognized her infant, and began to converse rationally, but had no recollection of the events of the afternoon. During the night free evacuations from the bowels occurred. By the ensuing morning her pulse had sunk at least forty beats in the minute; and during the day the disease was so far conquered that my apprehensions of danger were removed. The remaining treatment was similar to that of my other cases during convalescence; viz. a large dose of quinine at the periods most likely to anticipate any attempt to form fever; and purgative pills, sufficient to procure copious, consistent evacuations from the bowels, with enough afterwards to keep them merely regular." It is worthy of remark that these large doses diminish the frequency of the pulse when under the influence of febrile excitement. Dr. P. closes his paper by observing. "My observations so far, exhibit the following as one of the successful modes of treating our autumnal fevers, whether congestive or inflammatory. Bleeding whenever the symptoms require it. A dose of from six to twelve grains of sulphate of quinine, every two or three

hours, at any period of the fever, until its symptoms in the pulse and skin are subdued. Then purgatives to obtain copious, consistent evacuations from the bowels, until they regain their usual power. Subsequent attempts to form fever should be counteracted by a large dose of the quinine.

The best quantity for a dose, (from eight grains to sixty, or more,) the best intervals of exhibition, (from one hour to twenty-four, or more.")

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An essay, by Dr. W. P. Dewees, on leucorrhœa. Although we consider this paper as interesting and valuable, yet we do not perceive any thing new in his account of the treatment.

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Dr. Jackson's paper on vitality and the vital forces, though valuable, is not of a sufficiently practical nature.

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Dr. Hays' paper on purulent ophthalmia is an elaborate one, and does not admit of being condensed into a small compass.

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George W. Carpenter's interesting experiment on the extinction of mercury by trituration, have already been noticed in the foregoing number of the recorder.

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Spontaneous cure of Aneurism.—William Darrach, M.D. gives a highly interesting description and plate representing an aneurism of the upper curvature of the aorta, involving and obliterating, the arteria innominata and left carotid, at their origin; the right carotid and subclavian, however, remained pervious, and were supplied with blood from the left subclavian by means of an enlargement of the inosculating inferior thyroid arteries; the aneurismal sack was filled with a coagulum, made up of numerous strata. Mr. D. conjectures that an enlargement and union of the upper thyroid arteries, would have been found, but circumstances prevented the continuance of the dissection, he further observes that "the manner in which the vessels on the sac are obliterated deserves attention. The left carotid was closed at its origin, and contracted so as to form an inverted cone. It contained a coagulum changed into a whitish fibrous mass, which adhered firmly to the coat of the artery." In the innominata, "the sides of the artery were pressed together laterally and a union of them had taken place from the effused coagulable lymph," from these circumstances, he was led to the conclusion, that "the efficient means of making an artery impervious, is a conical change in the form of the vessel. All that is necessary to the formation of an internal coagulum, is to convert the cylindrical artery, into a conical vessel." This is best done according to the suggestion of Dr. D. by passing a broad animal ligature round the artery and then tying it over a pledget of cerate plaster, sufficiently tight to bring the sides of the artery in contact. Thus avoiding the division of the internal coat, and imitating the operations of nature as portrayed in the case of the innominata in the above description. This method of Dr. D's is an improvement, on that recommended by Scarpa.

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Dr Daniel J Carroll's paper on the morbid effects produced by drinking cold water; though the facts contained in it are valuable, they are not remarkable for novelty.

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The Topographical and Medical sketch of Tinicum Island by George F. Lehman, MD. is highly useful. The more this kind of knowledge is diffused the better; it will doubtless enable future writers upon the diseases of the different sections of our country, to explain some seeming paradoxes, to make useful comparisons, and at any rate to reason with greater accuracy upon the causes, &c. of those diseases.

A case of fractured skull with loss of a portion of the brain, in which a "perfect recovery ensued," is reported by Alexander Jones, M. D. Lexington, Georgia.

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A case of tumour successfully extirpated by David L. Rogers, M.D. It was of very large size, situated upon the neck and shoulder, nothing unusual however, in its nature. Dr. R. classes it according to Abernethy, as an Adipose sarcoma.

ART. III. *The New York Medical and Physical Journal, for July, August, and September*, 1826.

THIS number commences with the continuation of the paper of Dr. Fountain, on the Diseases of Irritation, which we noticed in our last number.

We select from it the following case: A young man, ætat. 23, had been affected eighteen months with the nervous headache The pain commenced in the morning, and continued severe until evening; his appetite was bad, pulse 130, and spirits depressed. He had been previously treated by bleeding, blistering, and purging, had taken tonics, antispasmodics, &c. without relief. Dr F. "gave him pills containing each one quarter of a grain of arsenic, ordering one morning and evening Finding partial relief, he unadvisedly doubled the number, and took occasionally a laxative; and at the expiration of twenty days, found himself free from his malady."—One grain of arsenic a day, for several days in succession, is a *tolerably* large dose; we must confess we should be unwilling to adopt the doctor's practice, notwithstanding "no ill consequences resulted."

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Dr. T. Romeyn Beck gives a detail of two trials for murder, which are interesting in a medico-legal point of view. In the one case, death was produced by a blow over the region of the stomach; on examination after death, some small red spots of a stellated form were visible on the internal surface of the stomach, apparently effusions of blood from some of the smaller vessels. Cases are cited were no morbid change was perceptible, nor was there any mark of violence; death is to be accounted for in these cases, to the sudden shock communicated to the brain through the medium of the sympathetic and par vagum nerves.

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Professor Holmes, of Montreal, relates the case of an infant, in which the œsophagus and trachea communicated; it lived ten days, and whenever applied to the breast, it drew the nipple as usual, but was immediately seized with symptoms of suffocation, which were only relieved by throwing up the milk it had taken.

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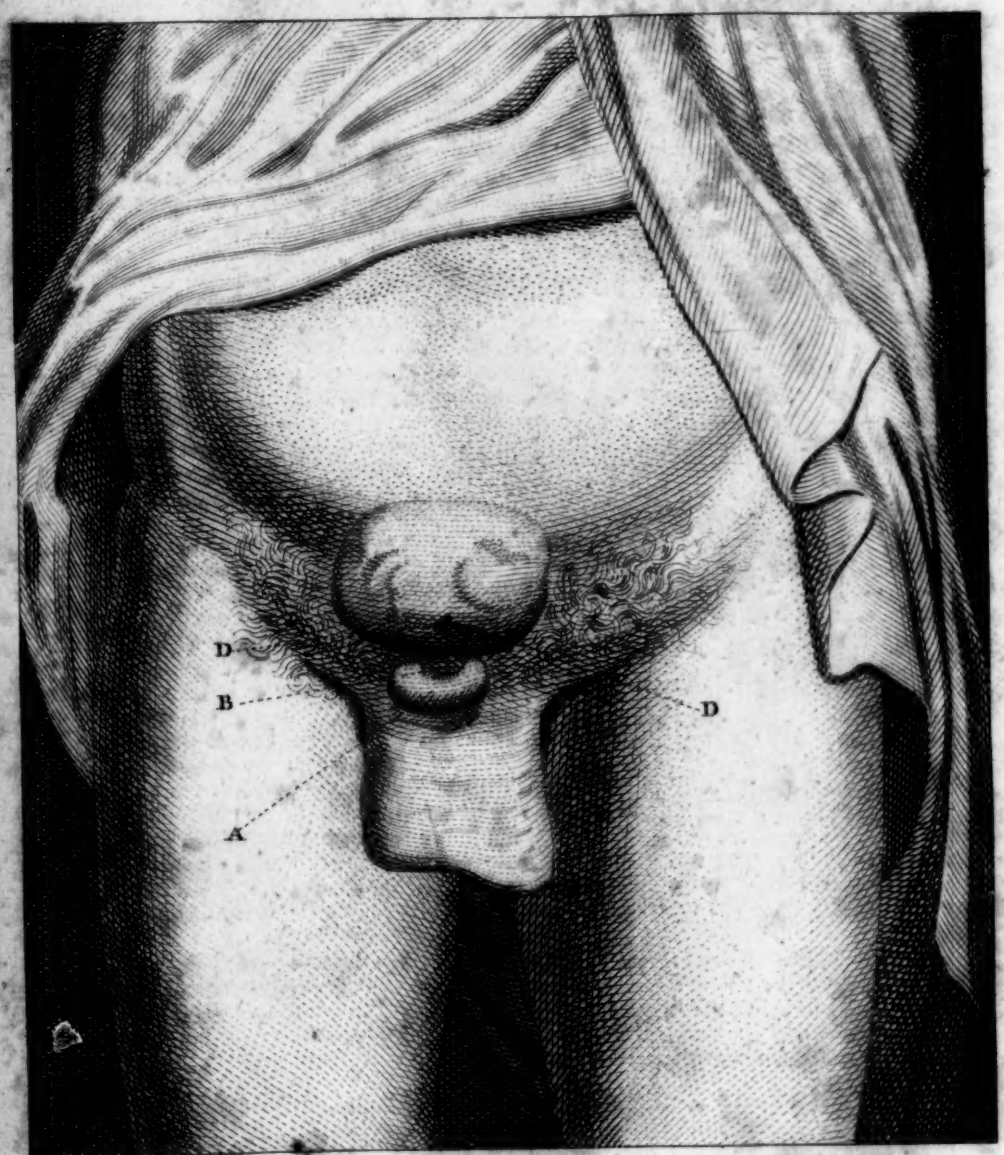
Article 4, is *a case of Malformation*, (Maleformation,) by a Dr. Drake, of New York, which we recognise as one already described in the Recorder, and published in our third volume. On reading this *second edition* of Robinson's case, we could not prevent the obtrusive idea from disturbing us in its perusal, that the writer had been very much at a loss for something about which to write, when he determined upon its republication. Nor could we fail to be struck with the unhandsome manner in which the case, as published in this journal, is adverted to. Any one who will be at the pains to compare the two descriptions, will, we are satisfied, derive a much clearer, and more definite idea of the external appearance of this maleformation, from the description given in the Recorder, (Vol. III. p. 515,) than from the obscure and jumbled account presented us in the Medical and Physical



J. Webster Jun^r Del.

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Journal; and as to the plate, (to say nothing of the execution of this precious specimen of the fine arts,) we are half tempted to believe that the drawing from which it was taken, was never an original delineation from nature, but nothing more than an *imitation* of the one that we ourselves have given to the public. Our reasons for this (perhaps illiberal) suspicion, is the remarkable resemblance between the two engravings. It is not merely a general resemblance, which might be expected in two accurate and faithful pictures by different hands; but the likenes of a servile, though badly executed, *copy!* The size, the proportions, the very drapery, (this being scarcely more than an inverted representation of ours,) and even the minute shading, (and particularly upon the tumour,) are so singularly alike, that we may appply to them the description given us by the poet of the twin sisters.

"—— Facies non omnibus una,
Nec diversa tamen."

In the description given in the Recorder, the tumour is said to have been "about the size of a goose's egg," and our plate represents it in that proportion. *Mark this!* At the time when Dr. Drake describes it, he tells us "its circumference at its base measured *nine inches and three fourths*," which would make its diameter three inches, and the whole tumour much larger than "a goose's egg." And yet, in his engraving, *there is not a corresponding enlargement* of the size of the protuberance; but it is represented in the very identical proportions of the drawing taken in 1814, and published by us in 1820. This curious inadvertence strengthens our suspicions. But we freely confess it is not every one who has an eye for proportions! and as there is something mysterious in the whole affair, we are perfectly willing that the analogy we have noticed, should be referred to the secret influence of the doctrine of "*correspondences*"

There is another circumstance which struck us forcibly on the examination of the monograph and engraving given us in the Medical and Physical Journal, to which we would call the reader's attention. It is, indeed, apparently a trifling circumstance, which some may think not worthy of criticism; but as it occurs in a description, which evidently professes to be the best that has ever been given, and which begins by an uncourteous slur upon the one already published, it seems proper to point it out to our readers, that they may be enabled to judge how much dependence should be placed upon its accuracy in other respects, when in one of considerable importance it is not even "*tolerable*." The writer tells us, as the writer in this Journal had mentioned before, that there was "*no trace* of an umbilicus;" and yet *in his plate* the umbilicus is fully represented! But, why should there not be "*fancy sketches*" in an engraving, as well as "lusus naturæ" in the human body?

As the case is certainly an interesting one to the curious in anatomy, we have thought that it would not be an unacceptable service to our readers, to subjoin the description given of it in this Journal in our volume for 1820; and to accompany this number with the excellent plate which we then caused to be made from a drawing, *taken from the parts*.

Case of extraordinary maleformation of the Genital Organs. By HENRY W. DUCACHET, M. D. of New York.

In November, 1814, a man, aged about thirty years, was committed to the Bridewell of this city. He immediately revealed to the medical attendants of the house, a singular maleformation of his genital organs. His penis was imperforate, destitute of a prepuce, not more than an inch and a half in length, and had an excavation upon the upper surface of the glans. A little above the insertion of the penis was a tumour, about the size of a goose's egg, of a granulated appearance, and of a bright red colour. This tumour

dilated and contracted irregularly. It was largest in the morning, and smallest at night. At the lower part of this tumour there appeared two foramina, of a size sufficient to admit a crow-quill, and from which the urine continually dribbled. He had a large inguinal hernia on each side. To defend the parts from pressure, he wore over them a tin plate, to which was appended a contrivance of his own, to receive the water as it dropped. His testicles were perfect, and remarkably large; and he confessed that he was frequently tormented with an inclination for venery. It is probable that the umbilical cord entered at this tumour, as there are no traces of a navel. In consequence of his confinement, he was not able to keep himself perfectly clean; so that the neighbouring parts were considerably excoriated by the acridity of the discharge.

Dr. Hosack, of New York, has in his possession a plaster model of a lusus of precisely the same kind, which he took from a man who exhibited himself to the students of the university of Edinburgh, during his attendance at that school, in 1792. Dr. Duncan has related several cases of the same nature; but the one just detailed is, perhaps, the only one that has ever occurred in this country.

REFERENCES TO THE PLATE.

A the penis. B the excavation on the glans. C The tumour. D D the mouths of the ureters, from which the urine continually dribbles.

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A case of Concealed Hemorrhage in delivery, is given by Dr. Wright, of Pittsfield. In the above case, the separation of the placenta evidently occurred thirty or forty hours previous to child-birth; not one drop of blood escaped externally, until the birth of the child; and along with a dead fœtus, was also expelled a coagulum of blood, weighing seven or eight pounds, with the placenta entire, and with the appearance of having been sometime detached. The usual remedies were resorted to, and notwithstanding very little blood was lost after delivery, she expired in about three hours after.

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A Quarterly Report of diseases treated in the New York city Dispensary, and a case of amputation of the arm at the shoulder joint, by J. Wagner, Esq., follow. The number concludes with some reviews, analecta, and *domestic* intelligence; among the latter we find a detail of some cases in which the carotid artery was tied for aneurism. The reporter, after stating the difficulty he encountered in performing these operations, takes great credit to himself for having been the first to perform them on children (one on a child nine years of age, and another on a child aged five years,) and expresses his "surprise that no surgeon has heretofore attempted to perform the same operation on a child," and states, that "the youngest patient on record is mentioned in one of Dr. Mott's cases, in which the patient was nineteen years old" For his information we would merely refer him to the Medico-Chirurgical Transactions, vol. ix. part i. p. 208, and he will find a case "*on record,*" where the operation was performed on a child about *six weeks old!* nor is any thing there said of its "*difficulty.*"

We consider it a matter of the utmost consequence, to have correct and faithful statements of the results of cases published; perhaps we may be thought somewhat sceptical, but we must confess we should wish to have stronger evidence concerning the result of the cases under consideration.

ART. IV. *New England Journal of Medicine and Surgery, for October* 1826.

MILO L. NORTH, MD. offers some observations on the proper influence of the pulse in its application to the diagnosis and prognosis of disease It contains nothing sufficiently novel, to authorize a further notice of it, though the observations are judicious.

In a post mortem examination of an interesting case of organic disease of the brain, by Dr. Jeffries, we notice the following fact. "The medula spinalis was very hard and remarkably contracted, not filling half the occipital foramen and upper portion of the vertebral canal. This gave the nerves arising from this part, a singular appearance, drawn from their apertures and kept constantly extended. The brain was firm and exceedingly vascular," &c.

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Galvanism in Epilepsy.—Usher Parsons, MD. relates a case of epilepsy, where a cure was effected by means of a galvanic apparatus, of a peculiar kind, after all other means had failed. The mode employed was that suggested and successfully used in a number of cases by Dr Mansfield of England, who directs "the removal of a portion of the cuticle," &c. of the size of a sixpence, by a small blister, from the back of the neck, as close to the root of the hair as possible, and a similar portion from the inside of the knee. To the sore on the neck, a plate of silver, varying according to the age of the patient, from the size of a sixpence to that of half a crown, is to be applied, having affixed to its back part a handle or shank, and to its lower edge, and parallel with the shank, a small staple, to which the conducting wire is to be fastened; this wire to descend the back till it reaches a belt of chamois leather buttoned round the waist, and to follow the course of the belt to which it is to be attached, till it arrives opposite the groin on the side where it is to be used; it is then to be passed down the inside of the thigh, and to be fastened to the zinc plate in the same manner as to the silver one.' On applying the apparatus, he directs that 'a small bit of sponge, moistened in water, and corresponding to the denuded place in the neck, be first placed directly upon it; over this a large piece of the same size as the metallic plate, also moistened, is to be laid,—and next to this the plate itself, which is to be secured in its situation by a strip of adhesive plaster passed through the shank on its back part, another above, and another below it. The wire down the back should be of such length as to admit of free motion of the head without moving the plate. A zinc plate is to be fastened to the wound at the knee, in the same manner; but in place of the second layer of sponge, a bit of muscle, answering in size to the zinc plate, is to be interposed. This apparatus will continue in gentle action for 12 or 24 hours. The sores now require cleansing and dressing, and the plates polishing. The best conductor is three or four fine copper wires; a chain will not do.'

The apparatus was applied, in the present case, according to the foregoing directions, and all other remedies were suspended; and not a single paroxysm of epilepsy has occurred from the day of the application to the present time, being more than two years. He discontinued the apparatus after it had been worn six months." Dr. M. was led to the employment of this plan, from some peculiar theoretical notions, relating to a supposed undue accumulation of galvanic or nervous fluid in the brain. The facts may be useful whatever we may think of the theory.

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Dr. Channing relates some miscellaneous cases, that contain nothing novel or interesting.

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Dr. David B. Slack, makes some interesting observations on the characteristic effects of the variations of atmospheric pressure upon the human body, for which we must refer to the paper itself.

ANALYSIS

OF

FOREIGN JOURNALS.

ART. V. *The London Medical Repository and Review, from June to November* 1826.

TREATMENT OF TETANUS IN THE TONGA ISLANDS.—Traumatic Tetanus, is said to prevail in the Tonga Islands, where the disease is termed *gita*.

If the accounts given of its treatment can be credited, it is at once novel and more successful, than any adopted among us; we shall therefore give a brief analysis of the method, there pursued.

It consists in exciting a violent degree of irritation in the urethra, and a hemorrhage from the part, which is usually effected by introducing a reed along the course of the canal. In the more aggravated forms of the disease, a seton is passed down over the end of the reed and brought out through an incision in the perinæum, thus converting the canal into a fistula. One end of the thread is allowed to project from the natural orifice of the urethra and the other from that made in perinæo; the thread being moved from time to time in the passage, excites violent pain and a free discharge of blood.

Our author states,* he has twice witnessed this latter operation, and each time with success

In these instances, the disease supervened to wounds in the feet; in both, the symptoms came on suddenly, and within a few days after the reception of the injuries, and in both, the spasms were extremely violent and occupied the whole body; but in neither, was the jaw *permanently* locked.

It is stated, the operation was performed, as soon as the disease developed itself, and on the fourth or fifth day, the thread was withdrawn (the symptoms having completely subsided.) Considerable pain and swelling of the penis followed, which however readily yeilded in a few days, together with a closure of the opening in perinæo.

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Mr. F. Bailey attempts "to prove, that the heart is the sole power concerned in the circulation of the blood." After divers preliminary observations, in which he endeavours to refute the theory of Dr. Whytt, who considered the blood vessels as having a share in this function; he proceeds to identify the action of the heart with a percussive force, and in support of his theory, illustrates the vast disproportion between the forces of percussion and pressure. After this prodigious effort to prove *percussion to be the mode of the heart's action*, he concludes, that as it is unphilosophical to assign more causes than are sufficient in the explanation of phenomena, ergo, this organ is the *sole cause* of the blood's motion.

* Mariner's Account of the Tonga Islands, by Dr. Martin, 2 vols. London, 1818.

Mr. E. Thompson, in a short notice of blisters in certain cutaneous eruptions, suggests the propriety of a removal of the thickened epidermis, previous to the application of remedies. He considers them as most applicable in varieties of ptyriasas, psoriasis, ichthyosis, &c.

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Walter James communicates a case of presentation of the placenta, fatal at seven months of utero-gestation. We do not think it of sufficient interest to enter into particulars. Various stimuli, pressure on the abdominal aorta, &c. were, employed to no purpose. Death immediately followed the extraction of the child.

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Dr. W. Shearman, in directing our attention to the distinction between rheumatism and inflammation, condemns the usual division of rheumatism into acute and chronic. He considers the chronic form as the *actual* disease; the acute a mere variety, occurring in vigorous habits, and so modified as to affect the whole system. We decline following Dr. S. through all his remarks on the characteristic differences observed between rheumatism and inflammation proper, as every one will doubtless admit their truth.

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E. Geoghegan in a letter to J. Abernethy, Esq., on strangulated hernia, attempts to establish the existence of adhesive inflammation in mucous surfaces. He strenuously advises the application of cold, which, says he, controls inflammatory action, abates sensibility, and condenses the volume of air contained in the strangulated portion of intestine.

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An interesting case of "extensive injury of the abdomen, by the goring of a bull," is detailed by Mr. W Dix, in which about three feet of intestine protruded, with a portion of mesentery and omentum. He recovered perfectly.

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Camphor Fumigation in Rheumatism.—M. Dupasquier has related several cases in the Revue Medicale, illustrative of the remedial effects of camphor in the form of vapour, in acute or chronic rheumatism. He was led to its adoption from the success attending M. Cheze's practice. He directs the patient to be exposed to the action of the camphorated vapour, in a proper fumigating apparatus, as that employed by Dr. Rapou. When this cannot be procured he directs his patient to sit in a chair, placed over a small furnace covered by a metallic plate. He is then to be enveloped in a blanket, tied close round the neck and extending on the floor. A teaspoon-ful of powdered camphor is then thrown on the plate every five minutes. This is soon volatilized and coming in contact with the surface, excites perspiration. This process may be continued three quarters of an hour or an hour, according to the strength of the patient. He is then to be wrapped up in the blanket and placed in bed, where the perspiration will be continued, and the absorption of the camphor promoted. Half an ounce is generally sufficient for one fumigation, which may be repeated three or four times a day. Remarks—The above method is not attended with uniform success, a result we might, a priori have anticipated. We have employed the camphor with decided advantage in the manner and form above detailed, and consider it a very useful and important addition to our therapeutric agents.

ART. VI. *The Edinburgh Journal of Medical Science. Nos.* 1, *and* 2, *for January and April*, 1826.

In presenting a review of a journal, which dates its origin from the commencement of the past year, some observations on the character of the original may not be thought superfluous. The editors, by remaining incogito, prevent us from pronouncing on the journal from their standing or reputation; there is, therefore, left to us the more judicious and candid method of deciding from the merits of the production alone.

It is intended to embrace, within its compass, a very extensive field, as it is proposed to exhibit "a compendious view of the progressive improvements and discoveries occurring in practical medicine, practical surgery, midwifery, anatomy, (human, comparative, morbid,) physiology, (human, comparative,) pathology, (human, comparative,) chemistry, pharmacy, botany, materia medica, and medical jurisprudence." The arrangement of the journal is into three parts, the first contains original papers, the second, reviews, the last, medical intelligence, foreign and domestic. The original papers appear to have been written by men of experience and observation; some of them, however, are not sufficiently practical. The reviews display considerable critical acumen; the observations are in general just, and the extracts judicious; and the selections under the head of medical intelligence appear to be good. Upon the whole, we consider this journal as promising to become a source of valuable information, worthy to be transferred to the pages of the Recorder.

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The paper, "On the indications and treatment of empyema, by Charles Hastings, M. D. Worcester," contains nothing new. The author appears to place great reliance on the stethoscope; he relates, at length, several cases where paracentesis proved successful, as also some where a spontaneous cure was effected.

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Affusions of cold water employed in a case of epilepsy, by Dr Haycraft, Birmingham. The effect was to lengthen the intervals, and diminish the duration of the paroxysms. Plumber's (compound calomel) pill was used in connexion with this mode of treatment in alterative doses. The case, however, ultimately terminated fatally.

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A singular case of phthisis laryngea, is given from Hufeland's Journal. This case is communicated by Dr. Fischer, of Oels, Silesia, for the purpose of disproving the healing qualities ascribed to herring milt, administered in this disease.

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Four interesting cases of glossitis, or inflammation of the tongue, illustrative of the mode of treatment most proper to be employed in this rare disease, are collected together from various sources. From these, it appears, that Dr. Olivet's method of drawing blood from the sublingual veins, is the most efficacious; this, together with general depletion by means of the lancet, purgatives, and clysters, as also local applications of an emollient or epispastic nature under the chin, and cold applications of nitre and sal ammoniac to the tongue, constitute the chief treatment. Leeches were also used under the chin, and to the neck and angle of the jaw, with considerable advantage; injections into the mouth, consisting of decoction of marsh mallows and nitre, or barley water and oxymel, or simply warm water, were also employed as adjuncts.

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A comprehensive view is taken of the state of the question, respecting the employment of caustic, as a means of arresting the pustules of variola, but as nothing new is offered, we pass it by without further notice.

Practice of Surgery.—A case of extensive tumour successfully extirpated from the muscular facia of the neck. By George Bell, Edinburgh. It arose from a diseased lymphatic gland, which, when removed, weighed four pounds and a half The mode of distinguishing whether the tumour is upon or under the facia, is worthy of recollection. "Tumours *above* the facia project considerably beyond the surrounding parts, and may be moved readily in all directions." "Tumous *under* the facia are harder, more diffused, more flat, and less moveable." It is important to be able to distinguish between them, as the former are removed with much less danger than the latter.

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Substance of observations communicated to the Philomathic Society, relative to a wound of the head, by Baron Larrey, in which it was made "evident, that the cauterization of the meningeal arteries did not, and indeed could not, be productive of any untoward accident, and that this is the most easy, the most simple, and surest mode of stopping the hemorrhage of such arteries as ramify in the furrows of the interior of the cranium." The mode employed was to "place upon the osseous furrows of the parietal bone, in which the two divided arteries were concealed, a small plate of bent iron, red hot, which instantaneously stopped the hemorrhage."

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The history of a case of supposed fracture of the neck of the thigh bone, within the capsular ligament, followed by bony reunion, is communicated by James Begbie, M D., &c. Edinburgh, together with a letter from Sir Astley Cooper, accompanying the specimen when returned to the owner, (who had sent it to him for his inspection,) expressing his doubts of its being a real example of this kind of fracture, together with his well known opinions on the subject of the practicability of bony reunion in such cases; that this esteemed surgeon is, however, mistaken, we think clearly proved by a case which came under the notice of Dr. Gibson, professor of surgery in the university of Pennsylvania; the specimen illustrating it is preserved, if we mistake not, in the Wisterian museum.

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Mr. Benjamin Bell states, in a very judicious paper on the subject, that he has found, in the inflammatory stage of gonorrhœa, camphor combined with hyosciamus, in the form of a pill, will produce the best effects in alleviating the agonies of chordée, without disordering the digestive organs. He has, in severe cases, pushed this remedy to the extent of one drachm of the former, to two scruples of the latter, in twenty-four hours! half the quantity is, in general sufficient; on the slightest symptoms of vertigo they are to be suspended. He also uses camphor, in the form of a liniment, to the loins, perineum, and groins

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A case of wound with protrusion of the stomach, communicated by Benj. Travers, F. R. S Surgeon to St. Thomas' Hospital, which was successfully treated, together with an abstract of numerous cases, which are derived from various authors, some of which terminated favourably. He arranges them under the head of simple and compound lesions of the stomach; the first class comprising those where that viscus is ruptured from strain, concussion, abscess, ulcer, scirrhous tumour, sphacelus, or chemical disorganization, or punctured by means of sharp bodies introduced into it, the external parietes of the abdomen remaining entire. The second class, those accompanied with an external opening, as from a wound with a knife, sword, bullet, or other foreign body penetrating the stomach, through the abdominal parieties, with or without protrusion of that viscus. He also cites cases where gastrotomy was performed with success in order to extract foreign bodies.

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A case of spontaneous dislocation, by J. Gibbon, M. D., Swansea. This

accident took place after the reduction of an ordinary dislocation of the hip. The displacement occurred twice, and they were at last compelled to secure the man firmly to the four corners of his bed, which perfectly succeeded; it was presumed that the upper part of the acetabulum was broken down in such cases; we would venture to suggest the application of the apparatus of Desault, as for a fracture of the neck of the femur.

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A paper on aneurism, by Robert Allen, Esq. F. R. S. E., containing several cases of this affection in different parts of the body, successfully operated on by him; such cases have been multiplied so much of late years, that it would be needless to give them in detail here.

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Under the head of midwifery, we meet with a very interesting case of difficult parturition, followed immediately by emphysema, by William Campbell, M. D. Edinburgh. It yielded to gentle means, and probably arose from rupture of some of the air cells of the lungs during the violent efforts accompanying labour.

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Also, a case extracted from a Prussian Journal, in which the Cæsarian section was performed with success for both mother and child. By Francis Antony Vonderfuhr, surgeon, Dahlen.

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A case of male-formation of the urinary and genital organs of a male child. By Ebenezer Gairdner, M. D. Edinburgh. A little below the umbilicus, the ureters opened through the parieties of the abdomen, like two papillæ towards each side of a considerable tumour, very much inflamed and excoriated by the stillicidium of urine below this tumour; an irregular mass presented, appearing to constitute an imperfect penis. Under this, a full sized scrotum, containing only the left testicle; on dissection was found a separation of the recti muscles, and deficiency of bone at the symphysis pubis, the former filled by tendon, the latter by fat and cellular membrane; internally the tumour formed a hollow, or cup, lined with peritoneum, and containing a portion of small intestines; there were two ureters from the right, and one from the left kidney; the former joined before they opened externally; "the right testicle was found at the groin imbedded in fat." The vasa deferentia were traced.

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A highly interesting paper is presented on the causes of the insalubrity of the air in the vicinity of marshes communicating with the sea, by Sig. Gaetano Giorgini, of the Academy of Lucca: in which it is demonstrated, "*that the presence and mixture, more or less prolonged during the summer, of the waters of the sea in the marshes of fresh water, constitute in the climate of Italy, the principal cause of the malady accounted so terrible, which is well known under the name of Malattie die cattiva aria.*" To the south of the Ligurian Apennines lies a marshy region, which was formerly depopulated annually by disease, and which has been rendered healthy by the construction of flood gates, in the canals communicating with the sea, so that the marshes may be drained without being inundated by the salt water. Whenever the flood gates have got so out of repair as not to effect this object, the malady has returned, and has again been expelled from this district, by mending them. Sig. Giorgini asks, "is it from a simple mixture of fresh and salt waters, that the pestilential miasmata which infect the air of these countries, develop themselves; or do these principles rather result from the destruction and corruption of certain vegetable and animal tribes, which, being unable to exist in sea water, are destroyed by the introduction of this fluid into fresh water?"

Samuel Hibbert, M. D., &c., in a very ingenious paper, endeavours to establish the identity of the "*Morbus Gallicus*," with the yaws and sibbens, which he classes together, under the name of frambæsia.

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William Howison suggests an improved mode of treating epistaxis, as it occurs in aged individuals. He employs a strong decoction of oak bark, in which sulp. of alumine has been dissolved, (the formula is as follows; ℥ss. of oak bark may be boiled slowly in lb.iij. of water, down to lb.ij.; then strain, and add ʒss. of sulp alum.) This is to be thrown, with freedom, by means of a large syringe, from the bleeding nostril into the back part of the mouth; he speaks of it with the highest confidence. He, however, makes one objection to the solution of alum, that it is liable to be retained in the antrum maxillare, &c., and may there form crystals of the salt. "Dr. Hamilton mentions a case, in which he introduced his hand into the uterus of a woman, who had been long using this solution as an injection, and he found that cavity entirely filled up with crystals of alum."

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A case of partial spontaneous fracture of the femur, in a child, is communicated by Mr. Benjamin Bell. He observes, "the periosteum was entire, except in the fore part of the bone near its middle, where a small carious spot, about the size of a split pea, was perceptible."

"I now made a longitudinal section of the bone by means of a sharp thin bladed knife, and discovered a fissure extending from the carious spot obliquely downwards." The child was two years and six weeks old, of a rickety and otherwise unhealthy habit of body.

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William Campbell, M. D., relates a case of emansio mensium, from interception of the vagina by a preternatural membrane. This was divided; the parts again became united, and a return of all the symptoms was the result. The operation being again performed, the precaution was taken to introduce a small wax candle, which was attended with success. Dr. C. relates also a singular case of mal-conformation of the vagina, which is worthy of notice. It was communicated to him by Dr. Hodges now a practitioner in the West Indies, in these words. "During Dr G.'s absence, I met with one of those unusual cases in midwifery, in which conception had taken place, although the vagina was so small as only to admit, with some difficulty, the female catheter. There were no nymphae, no appearance of clitoris, or urethra. The only parts visible were the passage already described, and the labiæ pudendi. This state of the parts was not discovered until many hours had elapsed after the commencement of parturition at the full time. At last an incision was made in the usual course of the vagina, of sufficient extent to admit the hand in a conical form, without difficulty. The passage was contracted to the extent of about two inches, beyond which it was of the usual capacity. The woman was much exhausted, uterine action ceased entirely, and after considerable delay, it was at last found necessary to use the perforator. The patient, however, had a rapid recovery, and shortly afterwards resumed her labours in the field."

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Dr. Knox, F. R. S. E., in a very interesting paper, endeavours to establish the principle, that monsters which are deficient in some essential part, owe their origin to the proximity of the placenta; this he illustrates by means of a deformed fœtus, now in the musuem of Dr. Barclay. The following is extracted from this description.

Abdomen.—The muscular parietes of the abdomen are wanting, but the peritoneum is entire. To this peritoneum is attached, by numerous short strong membranous cords, a placenta sufficiently large and natural, excepting

that its lobules are separated more completely than usual, which may be the result of maceration in spirits.* The vessels forming the umbilical cord are present and distinct, but this cord is only a few inches in length, and does not differ in appearance or length from the membranous attachments spoken of.

On raising the placenta, a very remarkable distribution of the urinary and genital organs presents itself. There is first a large open cavity, the surface of which is evidently integumentary. Towards the margin of this cavity, we observe two small apertures, ascertained to be the common opening of the ureters and vasa deferentia. Outside the cavity, and situated on the common integuments, is a papilla, having some distant resemblance to, and occupying the situation of the penis. An opening exists immediately below this, leading into the large cavity already described, sufficient to admit a common silver probe. This great external cavity, which the continental anatomist will, no doubt, declare to be analagous to the cloaca of a bird, and in which exists the openings of the urino-genital organs, leads directly, and by a very wide aperture, into another cavity, which proved to be the termination of the intestinal canal, and may almost be viewed as the cœcum deprived of a portion of its parieties; for the stomach and small intestines are naturally enough formed, but the latter terminate in the mucous cavity I speak of, and in such a way that this cavity evidently occupies the situation of the cœcum. The irregular honey-comb appearance of this second cavity, and the numerous orifices of smaller cavities, filled with mucous fluids, together with the termination of the small intestines, mark sufficiently its character.

The form of the liver is not quite natural, but the super-renal glands are present and large; the left kidney, (the only one noticed,) seemed quite perfect, as well as the corresponding ureter.

The pelvis and lower extremities are turned quite round, and are deformed; and from the loins there depends a vast sac, on opening which there was observed only a fringed cellular-looking substance, situated somewhat below a small aperture leading apparently into the spinal canal."

ART. VII. *Edinburgh Medical and Surgical Journal, for July*, 1826.

The first paper we shall notice, is on the cure of exostosis of the last phalanges of the toes, by excision of the diseased bone; by Robert Liston. This disease, an account of which is not to be met with in any of our surgical works, but has no doubt frequently occurred to their authors, who did not deem it worthy of a separate notice, affects the great toe most frequently, and is productive of great pain and lameness. "The tumour appears by the side of the nail, and is covered by thin shining integument; extreme hardness and immobility mark readily its character. The newly formed part is of the consistence of the bone from which it originates, and without any intermixture of cartilaginous, or softer substance. Some of these tumours adhere by a narrow neck, others have a broad attachment. They are of course met with of different sizes."

"In the first case, the tumour was cut off, but very soon appeared again, with all its former concomitants of pain and tenderness, increased by pressure or motion. Amputation was then performed in the middle of the second phalanx. Since then, I have been in the habit of recommending, and when it has been agreed to, performing the amputation at the joint, betwixt

* I will not affirm this. There is an evident tendency in the human placenta to divide into lobules.

the first and second phalanx;—an operation quite effectual, and more neatly, quickly, and easily done, than the division of the bone. No lameness ensues. The disease is of so exceedingly troublesome a nature, that patients will, in general, readily submit to any thing. The amputation of the whole bone appears to me preferable to any attempt at excising the diseased part only, as being less painful, more certain, and productive of as little inconvenience."

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A case of diffuse inflammation of the cellular membrane, arising from puncture in dissection, terminating favourably, by Mr. George Forbes. The favourable termination in this case, was probably owing to the intervention of *suppuration*. In most cases that we have met with, in which the patient recovered, this circumstance occurred, we have ourselves suffered from this affection, and its termination was in suppuration. The fatal cases that are to be met with in the various journals of the day, were attended with *erysipelatous* inflammation, and *gangrene!*

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We notice four cases of Hæmorrhœa Petechialis, related by Alexander Murray, which we think highly interesting, inasmuch as they occurred in individuals related to each other

Case I.—(July 13, 1824,) William Coutts, ætat. 13 years, had slight bleeding from the nose for a fortnight; three days since felt cold and shivered; no bleeding at present; sickness; thirst; face flushed; drowsy; some delirium; pulse 118. Epsom salts taken, which operated; has stigmatæ and vibices on the skin.

He had been attacked, when one year old, with hemorrhage from the gums, and has since been liable to returns of it, alternating occasionally with bleedings from the nose; worst attacks in summer; blue spots and dots always appear before he bleeds, and they fade away as the attack subsides. Bleeds profusely when wounded, and bruises of a slight character leave a livid mark. "At present, there is round one eye, a degree of ecchymosis, occasioned by an accident so trifling, that no pain was felt." Six years since, a hemorrhage from the nose stopped, and immediately after the blood issued from the crown of the head, "where no injury had been received;" the small opening from whence it proceeded was distinctly seen; saw him, for the first time, two years since, on account of hemorrhage among his teeth, which was stopped permanently in several places, by means of lint and strong pressure; attacks of late less frequent and severe than formerly—"has just been bled from the arm, and though two veins were opened, not more than six ounces of blood were obtained,"—seems easier; five grains of calomel in the evening, and to-morrow an ounce of epsom salts; cold water to the shaven head frequently.

15th. Better—head-ache gone; pulse 60, not quite regular; no thirst; appetite and strength improved this morning—vibices fainter; no difficulty in stopping the wounds in the veins; nose bled a little; coagulum of the blood drawn on the 13th, rather loose, covered with a pale red film—castor oil to be taken.

17th. Is now going about; complains only of weakness. Gums pale—said to be sometimes white and sometimes swollen—cold bath, and a weak solution of epsom salts. The mother of this boy had lost three children from a similar disease. We shall not give the outline of them. Another case is detailed, of a boy related to this woman, who died of this complaint. As there is nothing peculiar in it, we omit it.

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Thomas Hope, Esq. surgeon, makes some highly interesting observations on the powerful effects of a mixture of *nitrous* acid and opium, in curing dysentery, cholera, and diarrhœa. Accident led him to discover the efficacy of this combination, twenty-six years ago. Since that period he has used the remedy with unvaried success. The form of prescription is as follows:

R. Acid Nitrosi, ʒi.
Mist camphor. ʒviij. Misce et adde.
Tinct. Opii, gtt. xl.
Sig. One fourth part to be taken every three or four hours.

"No previous preparation is required for taking it, nor any other care whilst taking it, except keeping the hands and feet warm," avoiding exposures to currents of cold air, and making use of warm barley water or thin gruel, and a diet of sago or tapioca. We shall present the reader with a comparative trial, made by Mr Hope, in his own words. "About midsummer, 1819, twenty-six men were invalided for dysentery from the West Indies, and divided into wards, eleven in one, and fifteen in the other; and it was agreed that those in one ward should be treated in the usual manner, and those in the other with nitrous acid. The ward with the fifteen patients fell to the acid mixture, and twelve were recovered, and three died, and of the eleven treated in the ordinary manner, three recovered and eight died." The author tried the nitric acid unavailingly; it being the *nitrous* acid, which in combination with opium produces such beneficial effects.

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From an official correspondence on Mr. Henderson's method of treating the Indian cholera, we learn that Mr. H. relies on large and repeated doses of castor oil, and from his statements, his success must have been unparalleled; his plan was, to "place the patient on his back, in a charpey, with his head particularly low; I commence, he says, with 1½ ounces of castor oil, and continue giving one ounce every twenty minutes, until the bowels are freely opened by it. The patient is kept in this position by force, and not allowed until after the operation of the medicine, the slighest motion upon any account whatever. If vomiting should, notwithstanding this, take place, (as it once or twice did in the early stage of my practice, but which never now occurs to me,) a few drops of the laudanum may be given, but, for the reason above mentioned, the less of it the better. Stimulants may be given after two hours; not, however, before the castor oil has had time to pass through the smaller intestines, when the favourable symptoms become apparent, long before any evacuation has been produced by the medicine. It is to this fact, in the operation of the castor oil, that I have alluded, as a proof of the disorder having its existence in the smaller intestines; for, if it be allowed that the intestinal matter passed through equal lengths of the canal in equal times, the disease must be seated in them. No native has as yet died under my care in this complaint, who had taken the remedy, and survived an hour afterwards; and those (two in number) who did not survive the hour, had been affected with the disease twenty-four hours, and were in the agonies of death when brought to receive the remedy. Relapses often occur, and must be treated exactly in the same way; the patients, however, are never to be considered out of danger, until the bowels have been freely and thoroughly opened. The after-treatment of cholera is merely to keep the bowels moderately open, giving gentle stimulants to renew the healthy action of the bowels." His plan has been pursued with very flattering success by others.

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A highly interesting case, in which the head of the humerus was successfully excised, is detailed by James Syme, Esq. the following description of the operation, may not be deemed uninteresting. "The patient being seated on a chair, I made a perpendicular cut from the acromion through the middle of the deltoid, extending nearly to its insertion, and traversing one of the old sinuses. Introducing my finger into the opening thus made, I felt that the disease was chiefly, if not entirely, confined to the head of the humerus; and, therefore, cutting in a semilunar direction, backwards and upwards, from the lower extremity of the first incision, I formed a large

flap from the external or scapular portion of the deltoid, which being held up, exposed the joint so far, that I was able to insulate the head of the bone by means of my finger, and then to detach the scapular muscles from their connections with the tuberosities, when, the arm being brought forwards, I easily protruded the head of the humerus, embraced it in my left hand and sawed it off without any injury to the soft parts.

I next examined the scapula, and had the pleasure of finding the glenoid cavity perfectly sound, though deprived of its cartilage. The coracoid process, also, seemed to be free from disease; but, as the acromion, at its extremity, was bare and rough, I removed a portion of it with the cutting plyers.

On making the first incision, there was a rather alarming gush of blood; but the only artery of consequence divided during the operation was the posterior circumflex, which Dr Dease compressed with his finger until I completed the excision. This vessel was now tied, and then the flap being replaced, was retained in perfect apposition by five or six stitches of the interrupted suture. The only additional dressing consisted of some lint compreses, and two or three turns of a bandage."

We cannot trace the case in its progress after the operation, to which the disease of the bone gave rise, it had existed seven years, rendering the arm entirely useless, and threatening to destroy the patient's life, it was effectually remedied by the operation, and though the arm was shortened yet "the shoulder joint was susceptible of distinct motion, in all directions by means of its own muscles."

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Dr Vernon, relates an interesting case of obstruction to the passage of the blood through the right auriculo-ventricular opening of the heart arising from a fibrous concretion, which was entangled under the cordæ tendinia of the tricuspid valve, the individual in whom it occurred, had a short time previous to this attack, which terminated his existence, ran three miles, over a rugged and mountainous part of the country, he was suddenly siezed with a violent pain in his head and loins, while engaged at his work, in a spinning factory. Dr. V. states, that "on entering the house, he observed him sitting on a chair before the fire in a very stiff constrained attitude, his hands were forcibly pressed against his temples, and at intervals corresponding with each systole of the ventricles, he clenched them in agony at the repercussions of pain, which that alternative movement of the heart caused in his head.

His countenance was of a pale, leaden hue, with a remarkable turgescence of the subcutaneous veins of the forehead, face, and neck. It resembled much that of a person in an epileptic fit, the similitude being heightened by a spasmodic rigidity, affecting the muscles of the jaw, accompanied with a large quantity of frothy saliva which foamed over the lips. He had become very deaf since the attack, but gave very correct answers to all questions that were put to him, though in a hurried, irritable manner.

Dr. V. believes, the concretion to have been separated from the blood during life, and to have caused death by plugging up the passage from the auricle, into the ventricle, agreeably to the opinion of Corvisart and Laennec.

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Mr. David Tait, relates a case, where the external iliac artery on both sides was tied, in order to cure a *double* femoral aneurism, as it is termed, that is, an aneurism in each groin, there is nothing worthy of notice in the details of the operations, except, that the patient recovered.

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Mr. Charles W. M. S. Graham, of Dalkeith, recommends the internal use of sulp. zinc in gleet and Leucorrhœa; having been frequently foiled, when using bals. copaibæ, pip cubeb, &c. he was induced to make trial of the sulp. zinc, made into pills with common turpentine; he commenced with

one of these pills, each containing 3 grains of sulphate, morning, noon, and night, and according to circumstances, he increased the dose to two pills, three times a day, which he never exceeded, "in every instance they effected a complete cure, without the aid of any injections," generally requiring eight or ten days, to produce this result; abstemiousness in regard to spirituous and fermented liquors, is necessary, as an opposite course will protract the disease: pushed the extent recommended, Mr. G. did not encounter any nausea, vomiting, or other unpleasant symptoms.

ART. VIII. *The London Medical and Physical Journal, from May to September*, 1826.

Mr. Higgenbottom relates four cases, illustrative of the efficacy of lunar caustic in the treatment of local inflammation. He states that the pain and swelling are speedily arrested; that in conjunction he usually directs an emetico-cathartic evacuant; which would appear to be no trifling auxiliary.

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Dr. G. Lefevre details the case of Louisa Subiron, ætat. fifty, affected with diabetes. Thirst so excessive, that she drank daily from twelve to fourteen pints: Skin dry, bowels constipated, impaired appetite, &c. She voided daily fifteen pints of urine, of a pale straw colour, having a sweetish taste and nauseous smell, and when analysed, found to contain saccharine matter in abundance. The treatment consisted mainly in venesection and the vapour bath.

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Case of "Scirrhous Affection of the Brain," by R. Wade. The patient, ætat. fifty-eight, was attacked with paralysis of the right side, which in three or four weeks yielded to active depletion. On resuming his former habits of intemperance a second attack followed in a few days. After in vain having recourse to depletion and mercury, he died. On examination an exostosis was discovered on the internal surface of the os frontis, pressing upon the dura mater so as to destroy its continuity and occasion a corresponding depression in the brain. No traces of inflammation observed here. In the upper part of the left lobus cerebri, a hard mass was discovered, of a light brown colour, which was readily separated, and when divided closely resembled the medullary sarcoma of Abernethy; the tumour chiefly consisted of striæ, of a yellowish white colour, and nearly of the density of cartilage, whilst the remainder had a dark gray aspect and was of a softer texture.

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Mr. G. Bell presents, in a late number of the Edinburgh Journal, an excellent practical paper on Wounded Nerves and their Treatment. After some preliminary observations, he proceeds to narrate the history of a young woman who had been bled ten days before in the median cephalic of the right arm. When he saw her, the fore-arm was bent to an acute angle with the arm, and the fingers firmly contracted. Every attempt at extension caused the most excruciating pain. The incision in the vein had united, but a gentle pressure occasioned great uneasiness. The pain extended downwards to the fingers and upwards along the inside of the arm to the axilla, clavicle, pectoral muscle and short ribs. When exacerbations of pain recurred, she was seized with startings, tremors, subsultus, &c., attended with febrile symptoms. Every remediate measure had been previously in vain resorted to, and tetanus was apprehended, if speedy relief was not obtained. An incision three inches in length was made over the vein, which being

displayed, was tied with two ligatures at one and a half inches from each other, and equi-distant from the wound in the vein. The portion of the vein thus enclosed, was divided, which caused instant relief, allowing the freest motions to the wrist and elbow-joints. Considering it unnecessary to pursue the case any further, we may add she recovered perfectly. Several other cases are detailed in illustration of this method of cure.

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In Dr. Stocker's paper "on the pathology of fever," we discover no new doctrine, nothing to fix our attention. He advocates the doctrine of the humoral pathology.

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An interesting "Case of Uterine Hemorrhage, successfully treated by the Operation of Transfusion," is communicated by C. Waller, Esq.

Mr. W. very ably and successfully defends the propriety of the measure, and observes that "medical evidence is in its nature presumptive; it cannot be positive; and, therefore, when patients recover under any particular treatment, it is quite impossible to prove to demonstration that they would have lost their lives if a different plan had been adopted."

The patient, a female thirty-two years of age, of a very delicate and nervous habit, was much reduced by long continued nausea and vomiting. There had been a constant hemorrhage for seven hours before Mr. W. was summoned to her assistance; when he found the pulse nearly extinct, with cold surface, hippocratic countenance, and inability to articulate. On examination, the shoulder was found to present; the delivery was speedily effected by turning, and the placenta soon following, the hemorrhage ceased. Symptoms of collapse still continued, although stimulants were now exhibited. There was labouring respiration, a fluttering pulse; the coldness of surface increasing, and a distressing state of restlessness and anxiety.

After an interval of one hour and a quarter, when she had become totally insensible, and life almost extinct, the operation of transfusion was performed. The improvement was at once perceived, as the pulse, after several injections, became firmer, countenance more natural, and a gradual return of warmth and sensibility. Mr. W. concludes with observing, that the recovery of this patient establishes the conviction, that transfusion of blood in desperate cases of hemorrhage, is not that wild and visionary scheme, which some would make it appear.

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Mr. Richards details a violent case of headache, cured by purgatives. We value it for its practical utility and consider it as particularly deserving attention.

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Dr. Lee gives an account of scurvy, as it recently prevailed in the Crimea and on the northern shores of the Black sea. After describing the disease; he states, it was treated by mineral acids, vinegar and bitters; external fomentations with aromatic plants to the legs. Citric acid is not to be found in the country. He attributes the disease to the destruction of vegetation by the locusts; thus depriving the inhabitants of their usual supply of sour crout, &c. during the winter.

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Mr. W. Mackenzie details a "case in which the Secale Cornutum appeared efficacious in expelling Coagula from the Uterus." Its efficacy under these circumstances has long been established, hence the case in itself can possess but little interest. We cannot, however, forbear expressing our decided opposition to the views of Mr. M., who considers the ergot as a nervous excitant, acting through this medium on the muscular system; and in this way explains its modus operandi *in parturition*, which process, he says, is *chiefly* performed by the *diaphragm and abdominal muscles*.

Mr. M. is disposed to deny that the ergot has any specific or exclusive operation over the parturient female. He quotes Srinc, to prove its operation on males as well as females, by exciting a kind of formication, head-ache, vertigo, delirium, and opisthotonis He concludes by observing, since the convulsive ergotism so closely resembles the effects produced by nux vomica, that it is probable there exists in the ergot an alkaline principle analogous to strychnia.

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G. J. Guthrie offers some practical remarks on inflammation of the veins, after amputation.

He observes that it is the most fatal consequence of this operation and is most frequent in very irritable states of the system. It is either adhesive or healthy, irritative or unhealthy. The former is so mild it usually escapes notice. The latter as invariably fatal. When this condition of the veins follows amputation, the pulse quickens and is usually at 100 or 130, until dissolution; nausea, bilious vomiting, followed by fever, restlessness, anxiety, and a white tongue. The patient emaciates rapidly; skin is yellow and covered with perspiration; bowels irregular. Pulse becomes more feeble and frequent as the disease progresses. The patient gradually sinks; or he rallies a little and the appetite returns; but whilst he thinks he is convalescent, a slight accession of fever soon closes the scene.

There is no more pain than usual in the stump; nor any remarkable pain or uneasiness traced in the course of the vessels.

It tends to confirm (Mr. G. thinks) the pathology of phlegmasia dolens, as advanced by Dr. Davis, viz: that it depends on inflammation of one or more of the principal veins within and in the immediate vicinity of the pelvis."

He next proceeds to relate a case which terminated fatally and on examination, the vein on the face of the stump was found open and in a sloughy state; and as high as Pouparts ligament, the internal surface of the vein bore marks of inflammation of an adhesive character. Beyond it was an irritative or erysipelatous kind, terminating in the formation of purulent matter, lymph and blood. These appearances could be traced along the cava almost into the auricle.

Case of Sloughing Ulceration.—Treated at St. Thomas's Hospital by Mr. Travers. A. S. ætat. 17 was admitted with a small, irregular sloughy ulcer on each side of the perineum of three weeks continuance for which, simple remedies were employed unsuccessfully, as the sloughing recommenced and extended rapidly over the whole perineum and labia. The undiluted nitric acid was freely applied over the entire ulcerated surface and an opiate administered, under this treatment the sloughs gradually separated, leaving a florid granulating surface, which eventually cicatrized.

Several other cases are related, in which this acid exercised a beneficial influence.

Two cases analogous to the former, are communicated by Mr. Rose of St. James's Infirmary, in which repeated venesection was successfully employed.

A case of popliteal aneurism treated by Mr C. Bell, at the Middlesex Hospital, merits the attention of the profession in several important particulars. The operation in itself was attended with no unusual circumstances.

The moment after the artery was secured, the pulsation ceased; which however, returned in a few seconds, and in half an hour became nearly as distinct as before the operation. He died on the sixth day after the operation, and on examination, the femoral artery was found to be divided into two trunks.

On dissection, the sartorious muscle was found inflamed. "Just below the rt where the profunda was given off, the femoral artery was divided into o nearly equal branches. These ran dawn parallel to each other, to the rt where the artery passes through the tendon of the triceps muscle; here ey reunited. The ligature was found on the more superficial artery."

Mr. B. observed at his next clinical lecture, "the surprise is now, not that ıe tumour should have continued to beat, but how it should have ceased to eat by a ligature being put only on one of the arteries, either of which was ıully sufficient for the circulation of the limb."

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An unsuccessful case of Transfusion is related by George Jewel, Esq.

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Dr. Granville, in the presence of Messrs Brodie, Keate, Earle, &c., made an attempt to extirpate ovarian tumours, with a view to prolong the patient's life. An incision of 7½ inches, extended from above the umbilicus to the pubis; when on examination, numerous firm adhesions were discovered, between the tumours, parietes of the abdomen, and intestines, it was determined to suspend all further proceeding, the wound was accordingly closed by means of hare-lip pins, and at the end of a week she left her bed.

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The Medical public is highly indebted to Dr. A. B. Granville, for his valuable and interesting documents exhibiting the actual state of vaccination among 30,117 poor children. From authentic materials (as we are assured) a table has been constructed with much labour. An examination of which will lead to the conclusion; that vaccination is steadily advancing in public estimation; that inoculation for small-pox is quite rare. Which two causes will eventually eradicate natural variola, provided this course is steadily and perseveringly maintained; where small-pox has followed vaccination, it has been uniformly mild and harmless.

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Mr. J. Allen, after alluding to the specific actions of diseases and medicines, introduces to our notice five cases of periodical diseases cured by cinchona bark. Three of which were long-continued violent pains in the inferior extremities observing a periodical recurrence. The fifth was a case of rheumatic ophthalmia. The mode of administering the bark, was to give five or ten grains, in substance, every two or three hours, after premising active purging.

ANALECTA.

1. It has been well observed that there are fashions in medicine as well as dress; remedial agents and modes of practice, have almost all of them had their periods of celebrity, decline, and revival; and it is with them as it is with a new costume; if we take the trouble to look back upon the past, we shall mostly find that both the one and the other have been in use before, and have sunk into subsequent neglect.

This has been remarkably the case with the plan of employing large doses of calomel in the treatment of disease; a plan which was formerly in use, and which has been revived again within a few years.

Strange as it may appear, doubts are still entertained by many practitioners as to the propriety of this practice, notwithstanding the reiterated testimony in its favour, tending to prove its entire safety and decided superiority over the one now generally in use.

We are therefore induced to present the reader with a few extracts from Johnson's Medico-Chirurgical Review for April, wherein Dr. Cartwright's essay on mercury in syphilis, &c, published in the Medical Recorder for July, 1825, is reviewed in connection with a work by James Annesley, Esq. on the use of calomel in Indian diseases, both of whom recommend this medicine in large doses.

In order to show that this is not a new practice, we cite the following: "Horstius (Op. Omnia, vol. ii. p. 480) states that mercurius dulcificatus, or calomel, may be given in doses of one scruple, or half a drachm, 'ad viscidus humores magis attenuandos;" and D. Sylvius recommends it, in the same doses, as a purgative. Wepfer prescribed it in scruple doses, combined with other purgatives, in affections of the head; and so did Dr. Friend in Emansio and Obstructio Mensium. Schoeder, Junker, Geoffroy, and many others might be quoted, but enough has been said to show that the same fears were not entertained of full doses of calomel in those times, that now ring so often in our ears." We unfortunately pay too little attention and deference to the experience of those who have preceded us, and to this cause we should impute the decline of this modus medendi. The merit of its revival in *India*, is due to some naval surgeons on that station, and to our countryman, Dr. Cartwright, much credit should be awarded for having been the first to introduce it to the profession on this side of the Atlantic. He is not, however, if Johnson be correct, the first who has employed large doses of calomel in syphilis, as "several years ago, Mr. Cunningham, a naval surgeon, published some cases of syphilis which he cured in a nearly similar way, viz. by scruple doses of calomel, till the mouth became sore, when the chancres quickly healed, the whole period of cure occupying a much less space of time than in the ordinary way. Mr. Boyle, now surgeon of the Middlesex Infirmary in this metropolis, more recently published cases treated in a similar manner."

Dr. C. is, notwithstanding, entitled to the merit of originality, as he appears not to have been acquainted with these circumstances, and, moreover, has established the practice by repeated successful trials. Dr. Johnson observes, in continuation, that "the most remarkable circumstance in Dr. C.'s

paper is, a complete coincidence between himself and Mr. Annesley in this country, (Dr. C. having a month or two the start of Mr. A in point of publication,) respecting the treatment of fever, dysentery, and several other complaints, by means of scruple doses of calomel, given at such intervals as may prevent ptyalism, the object being to act on the bowels, and on all the excretories, without running particularly to the mouth." After quoting some of Dr. Cartwright's observations to show this more distinctly, he concludes thus: "The above will show a very striking coincidence between two practitioners, removed from each other a distance equal to the diameter of this earth. As both brought forward their observations at nearly the same time, there can be no reason to give the priority to either."

To conclude, we may observe, that our confidence in this practice in the treatment of the diseases of tropical climates, is fully established on the basis of actual and *personal* experience.

We have never witnessed the much dreaded hypercatharsis to arise from scruple doses of calomel, but, on the contrary, found them to be attended with fewer stools, and much less nausea, than the ordinary small doses.

They appeared to allay the irritation of the stomach, and their impression on the disease was generally most decisive.

We are promised some observations on this subject, from a highly respected correspondent, which we shall take an early opportunity of laying before our readers.

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2. Humoral Pathology.—M. Segalas reports the following results of several experiments made by him on dogs, with alcohol, and with the alcoholic extract of nux vomica, in order to solve the question of the *blood* being ever the seat of disease:—

1. Concentrated alcohol produces a chemical action on the blood during life.

2. Diluted alcohol, injected into the veins or bronchiæ, causes immediate drunkenness; but more slowly if directed to other parts.

3. That the effects of alcohol not introduced into the veins are, as to intensity and quickness, in direct relation with the absorbing faculty of the part where it is introduced, and quite independent of the nerves there distributed, particuiarly of those of the stomach.

4. That these effects are accelerated and augmented, or retarded and diminished by all circumstances which favour or impede the passage of the alcohol into the blood.

5. That drunkenness is dissipated in proportion as the alcohol leaves the blood, and more or less quickly as circumstances are more or less favourable to exhalation.

6. That the intensity of the effect of alcohol is not in proportion to the quantity which has been placed in contact with the organs, but with the quantity of this liquor which is actually in the blood.

7. Lastly, that profound drunkenness, and death from drunkenness, are coincident with a manifest alteration of the blood, and with less marked changes in the solid parts.

With respect to the alcoholic extract of nux vomica, M. Segalas found the following results:—

1. That this substance acts almost immediately after its entrance into the blood, and produces general or partial tetanus, according as it has been mixed with the mass of the blood, or only with a portion of it.

2. That when introduced in other modes than by the sanguineous system, it has no action except through the circulation; that its effects are independent of the nerves, and in direct relation, as to intensity and quickness, to the absorbing faculty of the part.

3. That the local phenomena of poisoning may be manifested independently

of the general enervation (*innervation,*) and have an absolute dependence on the local circulation.

4. Lastly, that many phenomena, quite inexplicable by fixed lesion of the nervous system, can be the consequence of a partial alteration of the blood; and can only be explained by the anomalous action which the altered parts of this fluid exercises on the portions of the nervous system with which they are placed in contact.

M. Segalas promises to investigate, in a second memoir, what diseases are to be considered as having their seat in the blood.

Arch. Gen. Avril.

3. Diseases of Children.—Dr. Gælis is adverse to opening the sanguineous tumours upon the heads of new-born children, because in his experience they have generally died!

"*Febris Cærulia,* attacks children from four to twelve months old and chiefly of the poorer class, who live on a scanty penurious diet. The disease attacks in paroxysms. The surface becomes suddenly livid, respiration difficult, and the pulse small, hard, and wiry. These paroxysms return at intervals, become more frequent, and at length pass into each other. The surface is covered with a clammy perspiration and death takes place suddenly. Dr. G. recommends ppts. of opium, with purgatives of cal. and rhubarb."

"*Convulsions.*—These are for the most part consecutive to inflammatory action of the brain; and Gælis never admits stimuli, as they only tend to aggravate the disease. Calomel is much employed by him.

"*Scarlatina.*—When the appearance of the eruption is interrupted, he directs the body to be bathed with tepid water, and condemns the use of sudorifics in the commencement of the disease.

"*Measles.*—When measles occur during vaccination, they both run their course undisturbed by each other.

"*Tinea Capitis.*—Its treatment is well understood; but Dr. G. asserts it to be of scrofulous origin; and Dr. Brosius remarks, that 'when the dried crusts of tinea, finely powdered, are rubbed by any one, upon the moist skin, true scrofula ensues.'" ibid.

4. *Hydriodate of Potash in Cancer.*—M. Greafe, jr. of Berlin, states that a female ætat 50, was admitted into the hospital, for cutaneous cancer extending from the upper part of the left mamma as far as the nipple. The ordinary means proving inefficient, M Graefe prescribed an unguent, consisting of Rose ointment ℥ij., hydriodate of potash ʒj. The appearance of the ulcer speedily improved, and the callosity of the margin diminished. When cicatrization commenced, the proportion of the hydriodate was doubled, and in nine weeks the patient was cured.—*Rev. Med.*

5. Case of local Self-Combustion.—It is related in Hecker's Annals for August, 1825, of a young woman, who, while occupied at her needle, perceived a sudden and extraordinary heat all through her body; and in her fore-finger a violent burning sensation, which became enveloped in a *blue flame,* to the extent of 1½ inches, emitting a *sulphureous odour.* The flame could neither be extinguished by affusions of cold water or a wet towel. By immersing her hand in milk during the whole night, the combustion ceased, She was afterwards admitted into the Hamburg Hospital, when the hand was found covered with vesicles, as in burns or scalds. The natural condition of the part, was eventually restored, without suffering any loss of substance, As the authenticity of this account rests solely on the young woman's evidence, our readers are at liberty to admit or reject it, as they may think proper.

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6. Chloride of Lime in Chilblains.—M. Lisfranc effected a cure in an obstinate case of chilblains, (where the back of the hands was extensively ulcerated,) by dressing the ulcers with plasters of cerate, perforated with

holes, over which compresses of charpie, dipped in a solution of the chloride, were applied. The dressings were renewed every twenty-four hours. *Rev. Med. Feb.* 1826.

7. Acupuncture in rheumatism of the heart.—A young lady, ætat. 18, was repeatedly attacked with erratic pains in the extremities, on the removal of which, she was seized with violent pain at the precordia, decidedly rheumatic and identical with those previously felt in the extremities, attended with palpitation, and sometimes a kind of extatic delirium. The attacks were sudden, and continued from fifteen minutes to three hours or longer.

Four years were thus passed, when the patient applied to M. Peyron, the reporter of the case, who describes the operation thus; the first needle was introduced with a rotary motion between the cartilages of the fifth and sixth ribs, with a direction towards the heart, without reaching it. The patient felt no pain, but soon after was seized with violent contractions of the limbs, and fell into the kind of delirium described by the magnetisers; and could not bear to be touched in the slightest manner. The paroxysm continued for ten minutes, when she complained of much fatigue and great pain. A second needle was then introduced in the same intercostal space, and a second attack followed, but without pain.

A third needle, and rather longer than either of the others, was introduced during the attack, between and below the two former; in a direction upwards and inwards, it penetrated the pericardium and no doubt reached the apex of the heart; the patient felt a sudden shock and the attack soon ceased. The peculiar and intense uneasiness excited by the last needle, the exact correspondence of its movements with the impulses of the heart, the severe pain attending its extraction, which was the only one followed by a few drops of blood, and was the most oxydated, all lead to the conviction of a direct communication with this organ. The needles were allowed to remain about 48 hours. From the period of their removal the pains ceased and have not returned, although the weather has been variable.—*Rev. Med.*

8. Uterine Hemorrhage.—M. Mojon, of Genoa has devised a plan for procuring the expulsion of the placenta in cases of uterine hemorrhage, and thus obviating the necessity of introducing the hand into the uterus. After pressing out the blood from the umbilical vein, he directs us to inject through this into the placenta, a quantity of cold water, slightly acidulated with vinegar. In a short time the uterus will contract forcibly, and the placenta be expelled. M. M. and others, have succeeded, even in cases of atony of the uterus. If necessary, the injection may be repeated, after allowing the first to escape, about a pint of fluid may be injected at a time.—*Rev. Med.*

9. Case of Polyphagia.—M Beaude relates, in the June No. of Nouv. Biblio. Médicale, a case of polyphagia, or indiscriminate swallowing, being at first the result of chance and subsequently of habit.

J. de Falaise, the subject of this affection was 62 years of age when this faculty was developed. Having drank pretty freely at a supper, his companions pursuaded him to swallow the landlady's canary bird, which he did in defiance of her tears and entreaties. He contracted with the manager of a petty theatre for five years, to receive a regular salary for swallowing in public, every thing that was given him. In his public career among a variety of articles, he swallowed 150 five-franc pieces; he afterwards swallowed crabs, eels, and even snakes. As might be supposed in process of time, he became affected with *gastro-enteritis*. From which he recovered, but eventually committed suicide by hanging. On examination after death, the pharynx and œsophagus were found capacious and readily distensible—at the cardia were three isolated tubercles with pedicles. The stomach distended, its muscular structure largely developed, and the pyloric orifice of unusual size, The mucous coat of the ileum was partially destroyed, and extensive cicatrices found in the cœcum.

10. Oil of Male Fern in Tænia.—The male fern has long held a high rank, in the list of anthelmintics. But on account of its bulk and the nausea it usually excites, its employment was necessarily limited. Impressed with the importance of obviating this difficulty, M. Perschier, chemist of Geneva, succeeded in discovering the active principle to reside in an essential oil. The following is an abstract of the report, presented by M. Gendrin to the Medical Society of the department of the Seine.

"This medicine, which is a fatty oil extracted by distillation from the æther, in which the powder of the root of the male fern has been macerated, has caused, in many cases, the expulsion of the tænia, without occasioning nausea, colics, or any other morbid phenomena.

"It is exhibited at bed-time, either in an oily potion, in pills, or incorporated in an electuary, in doses of 18 or 20 drops. On the following morning, a similar dose is given; and two hours after, two ounces of castor oil are administered. In most cases, the tænia is expelled in the course of the day; but if this does not occur, the same doses of the oil are given in the same way, and followed by a similar quantity of the castor oil. The fatty oil of fern has an ethereal and empyreumatic smell: its colour is brown, and its consistence rather greater than that of castor oil; it is, however, easier to separate in drops. Its taste is acrid, pungent, empyreumatic, and very disagreeable." *Propagateur des Sciences Medicales, January*, 1826.

11. Digitalis as a Remedy in Phthisis.—Dr. Neumann of Berlin, having employed digitalis in a variety of pulmonary affections, states as the result of his experience, that it is useless in tuberculous consumption, in abscesses following active hæmoptysis, or in *local* phlegmorrhagies of the lungs; but it is almost uniformly successful in those chronic catarrhs dependent on a state of erithism of the mucous lining of the bronchiæ. This form of the disease is sometimes designated chronic bronchitis, pulmonic catarrh and galloping consumption. In its exhibition he gives the following directions provided the diagnosis be accurately marked.

"1. The patient must be susceptible of the stimulant action of the remedy, this is often not the case. We may be sure the digitalis will not produce its effect where the pulse of the patient remains *uniform and frequent after he has taken it for several days.* It does not suit such persons.

"2. The medicine ought to be administered in a proper manner. To be good, the leaves even in the dried state, should be perfectly green, and free from any brown spots. Two ounces of the leaves should be infused in six ounces of boiling water, and the patient may take a table-spoonful every hour, until he feels nausea, or a sense of constiction in his throat, or flashing of the eyes, or irregular pulse. The use of the foxglove should then be interrupted for seven or eight days, in which interval the full action of the medicine is developed, the pulse remaining irregular, and the mucous secretion diminishing gradually. If the first trial does not remove it entirely, a second course may be commenced after a few days."

Propagateur des Sciences Medicales, February, 1826.

12. Aneurism from a Wound, cured by Valsalva's Method.—This interesting and valuable case is condensed from Le Propagateur des Sciences Médicales, for March, 1826. M. Antouard, a healthy female, aged eighteen, was wounded on the 18th of June, 1825, by a poniard, in the left carotid artery, below the superior extremity of the sternum, the instrument passing obliquely inwards and downwards. The anterior and lateral portions of the neck were enormously distended with blood, and syncope supervened. Four days after the injury was received, an aneurismal tumour was observed at the edge of the sternum, the surrounding effusion being greatly diminished by absorption; and at the expiration of a month, when she was first seen by Dr. Souchier, it was of the size of the two fists of the young female. The pulsations at this time were nearly equal over the whole surface of the tumour; but rather more distinct over the orifice in the vessel. The sur-

rounding blood was entirely absorbed. No pain was experienced, unless from the pressure of the swelling; from which cause, also, resulted a troublesome and continued headache. Dr. Souchier not believing an operation advisable during the warm season of the year, and on a tumour situated so much under the sternum, determined to fulfil the following indications: 1st. To lessen the quantity of blood, and thus to diminish the stimulus to the heart, the projectile force it exercises, and consequently, the rapidity with which the blood escaped from the ruptured vessel, and the impulse hence imparted to the sides of the tumour, preventing, in some degree, the coagulation of the blood. 2d. To increase the effect of general and local bleeding, by the use of *cold*, of *pressure*, and especially of the digitalis purpurea; that thus the force of the circulation may be lessened, the blood allowed to coagulate, and a radical cure be accomplished.

"Mademoiselle Antouard, determined to yield herself to this plan, and was directed: 1st. Rice-water, acidulated with lemon-juice, and an infusion of mallows, for *food* and *drink*. 2d. To employ frictions on the abdomen, and on the insides of the thighs, morning and evening, with eight grains of the pulverised leaves of digitalis, previously macerated for twenty-four hours in a sufficient quantity of saliva. 3d. To apply every day twelve leeches near the aneurismal tumour; and after favouring the flow of flood by emollient fomentations, to cover the part with compresses, wet with a saturated solution of the acetate of lead, to be frequently renewed, so as to be kept below the temperature of the skin. 4th. The effect of these means to be augmented by pressure, made by means of the base of a glass tumbler, fixed by the hands of assistants; and 5th. To be kept at rest, and in perfect silence.

"*Fourth day of treatment*, being 2d of Aug. 1825.—Pulsations more central: tumour very sensibly diminished; pulse less strong, and reduced from 86 to 74 in the minute; the menses, which had been suppressed for two months, appeared on the 31st ult., and still flow. *Prescription*, V. S. ℥xviij. next day, twelve leeches on the lateral parts of the tumour; gr. xxiv. of digitalis, in three applications through the day. Cont. ut supra.

"Aug 8th.—Patient tranquil; pulse 60, full, not active; face not flushed, but preserving a delicate tinge of red: headache now slight; no nausea; menses continued until the 6th inst. *Prescriptoin*, V. S. ℥xij.—fifteen leeches to-morrow: increase digitalis to gr. xxviij. daily; the rest ut supra. The tumour has diminished at least one-fourth.

"Aug 12th.—Tumour reduced to three-fifths of its former volume; pulse at 56; her nights are comfortable; has some headache, and lately, cardialgia; complains of hunger and weakness, and from the fatigue of her assistants, the pressure was made with a bandage less effectually than before. This was allowed, as the pulsations are weakened, and more and more central, while the elevation of the tumour is trifling. For fear her health might be injured, she was permitted to rise a little from bed, and to add to her rice-water some light jellies (crêmes) made from the same grain. V. S. ℥x, and every second day eight leeches around the tumour; digitalis increased to 32 grains daily; warm pediluvium for one hour, morning and evening; silence as complete as possible.

"Aug 18th.—No tumour visible; pulsations can yet be felt; the skin is thickened; pulse at the wrist is at 50. V. S. ℥viij.—six leeches every fourth day until menstrual period; digitalis reduced to gr. xx., and still to the same parts; continue the pressure; allow some rice jelly, vermicelli soup, gentle exercise; silence to be preserved, continue pediluvium, and relieve constipation by simple enemata.

"In fifteen days, Dr. Souchier again visited his patient. It required an experienced hand to distinguish, at the spot where the artery was cicatrised, an elevation rather more evident than over the rest of the artery. Pulse 48 per minute; hunger great, and the remedies now unpleasant. Most

of them were suspended, and fruit, and the white flesh of poultry, added to her diet list. The digitalis reduced to twelve grains a day. Compression, silence, and moderate exercise, to be continued as before. The menses appeared at the expiration of twenty-five days, and were more abundant than at the last period.

"At the end of a month no trace of the tumour was discoverable. The young lady had carefully increased her nutriment and exercise without inconvenience, and all remedial measures were now omitted.

"During the months of December and January last, she remained free from any inconvenience from the tumour; and the union of the parietes of the artery was therefore regarded as complete.

"In the above account, we have only to regret that the state of the artery above the tumour, before and after the treatment, had not been noticed. Perhaps this may be supplied by Dr. Souchier, in the commentary, which he proposes publishing on the above case."

13. Experiment upon Hydrocyanic Acid.—M. Dupuy at a recent sitting of the Royal Academy of Medicine at Paris, reported the result of an experiment which appears pretty conclusive in establishing ammonia as the antidote to Prussic Acid.—He introduced into the mouth of a horse, seven drops of pure hydrocyanic acid, diluted with twenty-four of distilled water. The horse soon staggered and fell; respiration became quick and labouring, nostrils dilated, mouth open; the tongue and eye-lids were convulsed. The animal became comatose and the pulsations of the heart very rapid, twenty minutes after, a drachm of subcarb. ammon. in solution was injected into the jugular vein; in a few minutes the animal rose, and after an hour, exhibited no farther evidence of its effect than slight convulsive movements with a quickened respiration.

In confirmation of the above statement, we subjoin the following brief extract from a late No. of the Medical Repository. Professor Fremi applied two drops of concentrated prussic acid to the conjunctiva of a rabbit, which caused violent convulsions and apparent death. An unsuccessful effort was made to resuscitate the animal by means of frictions. Resorting to Murray's antidote, they employed frictions with ammonia, without exciting the action of the heart. Ammonia diluted was next exhibited, and in a few moments vitality was restored. In order to test the accuracy of the experiment, the prussic acid was applied to another rabbit, without any attempt to recover it by the ammonia, and the animal died.

MEDICAL INTELLIGENCE.

Copy of a Letter to the President and Selectmen of Natchez, by Dr. Cartwright, and their Reply.

NATCHEZ, November 30th, 1826.

To the President and Selectmen of the City of Natchez:

Gentlemen,

By the enclosed letter from Philadelphia you will perceive that the statements I have made in two Essays published in the 33d and 34th Nos. of the Medical Recorder have been *denied* in general terms, or at least *doubted*. I have submitted the essays to your examination. By saying whether the statements therein made are substantially *correct or not*, so far as your knowledge extends, would, I think, be rendering a service to this city and to the Public. To support truth or to correct error on a subject which so deeply concerns both ourselves and others, may justly merit the attention of the city council. For it must be evident to every reflecting mind, that accurate histories of the rise and progress of yellow fever, embracing all the important circumstances which precede and attend it, is the only road nature has left open by which we can ever advance towards a knowledge of its causes, prevention, or cure. As facts are to be collected from different places and at different periods of time, it is necessary, in order to be *useful*, that they be *properly attested and supported*. As to the few I have collected, I know not who could be better qualified to decide concerning them, than the President and Selectmen of this city;—who, as constituting *the Board of Health*, must have had ample opportunities to acquire a knowledge of facts relative to the history of the yellow fever of 1823 & 1825, especially as one of their number is a physician who had an *extensive* practice *throughout* the progress of the epidemics of which I treat. *To them therefore I appeal.* The public could want no better *evidence*. For it is the best that can be had, and would have its due weight and influence, not only *in distant places but in future.*

Accept, Gentlemen, the best wishes and esteem
of your fellow citizen and obedient servant,
SAMUEL A. CARTWRIGHT.

To the PRESIDENT & SELECTMEN
of the City of Natchez.

NATCHEZ, December 4th, 1826.

At a meeting of the President and Selectmen of the city of Natchez, holden at the City Hall, on the 2nd inst.,

On motion of Dr. Samuel Gustine, the following Preamble and Resolutions were *unanimously* adopted by the Board, viz.

Whereas we, the President and Selectmen of the City of Natchez, having received a letter from Dr. Samuel A. Cartwright, with an enclosed letter directed to him from Mr. James Webster, Publisher of the Medical Recorder, Philadelphia,) and having *attentively* perused his essays on the malignant fever of this city, in 1823 and 1825,—Therefore

Resolved, That we deem it a duty we owe, not only to the *Author* and the *Publisher* of the aforesaid essays, but to the *public* in general, to state explicitly, (so far as we have been able to acquire a knowledge of facts relative

to the malignant fever of this city in 1823 and 1825,) we have no hesitation in saying that the statements of Dr. Cartwright are substantially correct: and being as we are, *acquainted* with the *respectable* standing and moral character of the author *at home*, we consider his essays well entitled to public confidence *abroad.*

Resolved, That Dr. Cartwright and the Publisher of the Medical Recorder be furnished with an official copy of the foregoing Preamble and Resolutions.

I certify the above to be a true transcript from the original.

Given under my hand and seal of the said City,
this fourth day of December, A. D. 1826.

(SEAL.) WILLIAM R. RICHARDS,
President of the Selectmen of the City of Natchez.

Attest,
WILLIAM H. CHAILLE, *City Clerk.*

Extract of a Letter from Charles Randolph, M. D. of ERWINNA, *to one of the Editors.*

The following interesting case occurred in the person of Judge *Prevost*, residing in Frenchtown, state of New Jersey, from whose own mouth I received the account, and from the high respectability of whose character the statement demands our most implicit confidence.

In the month of June, 1826, Mr. Prevost received, whilst labouring in his garden, a wound from the sting of a bee, about an inch and a half to two inches obliquely above and posterior to the external angle of the orbit of the eye. A short time after the receipt of the wound, his whole system seemed agitated with nervous tremors. These were succeeded by slight stiffness in the back of the neck, difficulty in speaking, and attended with a painful constriction at the lower end of the sternum. These symptoms, after existing for a short time, were followed by spasmodic contractions of the extensor muscles of the thighs and legs, with involuntary twitchings of most of the muscles of the upper and lower extremities. In this condition he was carried to bed, where he lay for some time in a state of indescribable wretchedness; his face and upper extremities covered with a cold, clammy sweat. From his feelings he now concluded that his dissolution was inevitably fast approaching. At this critical juncture a death-like sickness seized him, which, after a few severe struggles, brought on spontaneous vomiting. With this disappeared all his uncomfortable feelings, and with them all apprehensions of danger. The whole attack, from the commencement to the end, did not occupy more than half an hour—so that no physician saw him. Quere? If vomiting, accidentally occurring, will have the effect of curing an attack of Tetanus, originating from a wound probably of a nerve, What of the propriety of resorting to emetics for the same purpose? The torpor and insusceptibility of the system to the action of most medicines, I am fully aware of, but this I should imagine might be overcome by increasing the doses to a sufficient extent. At all events, in this recuperative attempt of nature, we have presented to us one of the strongest indications that she is capable of pointing out. Whether or not it would be advisable to imitate her in this one instance, I leave to future experience and the sounder part of the profession to determine. For my own part, should I meet with a case of Tetanus resulting from a wound, and more particularly if attended with any nausea, I should not hesitate a moment as to the safety or propriety of employing emetics, so as to produce active vomiting.

Extract of a Letter from John B. Gloninger, M. D. of Lebanon, Pennsylvania, to one of the Editors.

LEBANON, PENNSYLVANIA, October 28th, 1826.

In the evening of the 5th instant I was desired to visit Mr. J. S. Ætat. 28, residing about six miles from this place, who had been attacked with *epistaxis*, in the morning. Upon my arrival, I was informed that every means suggested had been tried in vain; that from the time he was seized with the hemorrhagy, until I saw him, it continued with but occasional and short intermissions; that he had lost upwards of two quarts of blood; and that it had been partially arrested several times by syncope, but as soon as he had revived, it returned with its wonted violence. Finding his strength exceedingly prostrated, his pulse scarcely perceptible, his breathing very much impeded, and his extremities cold, I applied immediately an epispastic on the back part of the neck. I was delighted to observe as soon as it began to vesicate, that its effects in this species of hemorrhage were in accordance with those witnessed by Drs. Archer and Street in the case related by them in the 1st vol. of the Medical Recorder. In short it acted like a charm. In order to obviate a recurrence of the disease, I kept the blister open for several days, and on the 9th I found him still labouring under considerable debility, for the removal of which I ordered a cordial and generous diet, and left him some tonic powders, which in a few days restored him to his accustomed health.

We have received a letter from our highly respectable correspondent Samuel Jackson, M. D. of Northumberland, in which he strongly recommends, more particularly to Physicians in the country, the use of the Spring Stirrup. As the Dr. justly observes, "the province of the physician is not merely the cure of diseases:—he is called upon by the obligations of humanity and professional duty, to obviate all the physical and moral evils that threaten our safety in the journey of life." When we consider the frequent occurrence of accidents from riding with the Stirrup in general use, we concur with him in his recommendation of the Spring Stirrup, which when properly hung, opens at the side and casts out the foot, let the rider fall or hang upon it in whatever way it is possible."

American Modern Practice, or, a simple Method of Prevention and Cure of Diseases, according to the latest Improvements and Discoveries, comprising a Practical System, adapted to the use of Medical Practitioners of the United States. To which is added, an Appendix, containing an account of many Domestic Remedies recently introduced into Practice, and some approved Formulæ, applicable to the Diseases of our Climate. A new Edition, improved. By JAMES THACHER, M. D. A. A. S. *Author of the American New Dispensatory, and Observations on Hydrophobia.*

"The young disease, which must subdue at length,
Grows with our growth, and strengthens with our strength."—POPE.

EXTRACTS FROM THE AUTHOR'S PREFACE.

SUCH has been the rapid progress of medical science, and such the essential improvements, effected by the zeal and talents of the professors and medical practitioners of the United States, within the last thirty years, that many periodical publications have been issued to record and promulgate important discoveries. These augment our materials for constituting a practical work, embracing, in a methodical view and condensed form, the principles of modern practice, every way adapted to the use of American physi-

cians, and calculated for the meridian of the present day. Such is the object which the author has for years had in contemplation; and the fruits of persevering labour and research have now resulted in a compendium of the most modern and approved modes of treating the diseases of our country, and of the most judicious application of the medicinal productions of our own soil. Throughout the whole course of this compilation, the most substantial authorities have been consulted, such as no one, it is presumed, will be disposed to impeach, and under whose influence, even the wise and learned of the medical faculty will not disdain to prescribe. Disclaiming all pretensions to theoretical explanations, and rejecting all hypothetical disquisition as fallacious, I have directed my views simply to rules of practice. In preparing the present edition, I have consulted the most approved European authors, especially the very elaborate and erudite production of Dr. J. M. Good, and the valuable works of Drs. Parr, Armstrong, Abernethy, &c. But as respects the epidemic diseases with which our own country has recently been visited, precedence has been given to American authorities as the surest guide to American practitioners; and those formidable epidemics which have justly excited the public interest and alarm, are portrayed in their true characters, with their medical treatment, according to the views of the most judicious and experienced physicians.

The author cannot but indulge the hope that this work will be found calculated to assist those practitioners whose retired residence may preclude them from the most ample sources of information, as well as the junior class who are about commencing their professional career, as the pledge and hope of the rising generation. To inculcate the high importance of a complete medical education, agreeably to the established rules of our universities and medical institutions, and to discountenance the attempts of the illiterate, who would thrust themselves into medical practice, are among the primary objects of this production. In the Appendix will be found a collection of formulæ for the convenience of the young practitioner.

PHILADELPHIA, October 13th, 1826.

TO JOHN P. WETHERILL.

Dear Sir,

In answer to your interrogatories concerning your *Extract of Peruvian Bark*, I have to state that my experience with that Medicine will authorise my highest approbation. In its preparation you appear to have attained the grand desideratum relative to the subject, viz. a neat, compact, and palatable medicine, containing all the medicinal virtues of the powdered Peruvian Bark, separated from its bulky, inert, and indigestible fibrous matter. Administered with the usual precaution, it has failed in no instance to establish its superiority over all of its congeneric preparations, in the present epidemic, in all cases in which the use of Cinchona was indicated.

I can experience no hesitation in saying, that Wetherill's Ext. of Cinchona will prove a very valuable addition to the Pharmacopœia.

Dose, one grain of the extract every hour or two during the apyrexia, increasing the quantity "pro re nata."

Respectfully your friend, &c.

R. HARLAN, M. D.

Medical Society.

At a late meeting of the Medical Society of Baltimore, the following gentlemen were proposed and elected honorary members:

Casper Tobias Baron Von Zollickkoffer, M. D. of Switzerland; Manuel Marvon, M. D. of Buenos Ayres; Thomas Nelson, M. D. Virginia; C. M.

Frailey, M. D. Ohio; L. Mackall, M. D. George-Town; P. S. Sugg, M. D. North Carolina; E. K. J. Hahn, M. D. Baltimore; George Wetherill, M. D. Moreau Forrest, M. D. Maryland; George Jacobs, M. D. Maryland; John W. Lewis, M. D. North Carolina; Francis M'Coy, M. D. Mexico; Thomas Roberts, M. D. Maryland; —— Yeates, M. D. Maryland.

By order,
WILLIAM KEERL, *Recording Secretary.*

Thermometrical Record, at Portland, Maine, for* 1826.

The following is an average of the Thermometer for each month of the year, noted at sunrise, at noon, and at 8 P. M. The Thermometer is placed without doors, where there is a pure current of air.

	Sun rise.	*Noon.*	8 *P. M.*
January	19 deg.	25 deg.	21 deg.
February	18 "	27 "	24 "
March	28 "	36 "	31 "
April	33 "	45 "	38 "
May	49 "	64 "	54 "
June	55 "	70 "	60 "
July	61 "	77 "	67 "
August	62 "	71 "	65 "
September	52 "	66 "	58 "
October	44 "	54 "	48 "
November	32 "	40 "	35 "
December	22 "	29 "	26 "

The mercury was the highest, July 11, 94 degrees.—Lowest, February 1, 24 degrees below zero, the lowest ever before known in this town.

Snow storms in January, six, - - - -	13 1-2 inches.
" " in February, six, - - - -	14 1-2
" " in March, two, - - - -	2
In April two or three flurrys of snow, - -	1
November, one, - - - - - -	2 1-2
December, two and flurrys, - - - -	8 1-2
Quantity fell in the year, - - - - -	42 inches.

The sun rose 177 mornings. Clear at noon 230 days. Set clear 179 evenings.

February 1, The Harbour frozen over to the island.
" 2, Broke up as far as the Red Buoy.
" 5, Harbour clear of ice as far as Long-wharf, and remained open the rest of the season.

Average temperature of each month in the years 1824, 1825, and 1826, as indicated by a Thermometer, hanging out of doors in the shade—observations made every day (except Sundays) at noon:

January	41.5	39.8	36.9
February	40.4	40.9	40.6
March	47.5	52.0	49.5
April	61.0	61.7	55.4
May	71.7	72.2	78.6
June	81.3	84.6	82.5

* Lon. 70° 20′ W., lat. 43° 39′ N.

July	86.2	88.7	85.4
August	80.2	81.7	82.7
September	73.0	71.0	77.8
October	62.8	66.5	64.2
November	50.3	49.0	51.3
December	43.1	38.6	40.6
Average of each year	61.6	62.2	61.2

Philadelphia Democratic Press.

Extract of Letter from Dr. Jameson of Baltimore.

I have thought proper, since my essay has been honoured by the highly respectable commission which approbated it, to support my matters of fact by second testimony. It is a mark of respect I owe my friends for their kind attentions to my feeble efforts to improve an important branch of medical science.

In addition to the testimony of Drs. Miller and Annan, I may add that of my young and excellent friend Mr. Benjamin Johnston, a student from York, Pennsylvania, who afforded me every assistance in his power, and who can, therefore, attest to the entire accuracy of all my experiments.

We certify that we witnessed the greater part, and the more important, of the experiments related in Dr. Jameson's Essay on Hemorrhage, and find them detailed correctly.

JAMES H. MILLER, M. D.
SAMUEL ANNAN, M. D.

BALTIMORE, January 8th, 1827.

RICHARDSON and LORD, BOSTON, have just published a Compendium of the Flora of the Northern and Middle States, containing general and specific descriptions of all the plants hitherto found in the United States, north of the Potomac. By John Torrey, M. D. Professor of Chemistry in the West Point Military Academy. We cannot too strongly recommend its perusal, by all those who are desirous of becoming acquainted with the indigenous plants of our country.

The annual election of officers of the "OHIO MEDICAL AND PHILOSOPHICAL SOCIETY OF CINCINNATI," took place on the last Monday of November, whereupon the following gentlemen were declared duly elected: JOHN MOORHEAD, M. D. President, J. G. LEE, M.D. Vice President, J. M. MASON, M.D. Secretary, DR. LAWRENCE, Treasurer. This society meets weekly, for the purpose of discussing Medical and Philosophical subjects. It consists, at present, of about one hundred members; and, from the manner in which the proceedings of the meetings are conducted, promises great usefulness and respectability.

FROM THE NATIONAL GAZETTE.

Mr. Editor—Believing that many of your readers would be pleased to have an opportunity of reviewing the meteorology of the past year, I have prepared the following statement, which exhibits, at a single glance, the mean temperature of each month, at sun-rise, noon, and three o'clock, P. M. It also shews the aggregate of mean temperature, the highest and lowest degrees at which the thermometer was observed, and its range; together with the quantity of rain, and the number of days on which the rain fell, in each month.

It has been frequently asserted that the climate of the United States is undergoing a gradual melioration, and some persons have been so satisfied

of the fact, as to theorize on its cause. But others are still doubtful, contending that seasons as mild, as those of late years, were experienced in the early days of the settlement; and intimating that a recurrence of hard winters may be expected. If such records as the present have no other value, they will, if continued, finally determine this question.

I remain respectfully,

THOS. SMITH, Gardener.

Arch street, between Schuylkill 7th and 8th streets.

1826.	THERMOMETER.									Rain Guage.	
Months.	Mean Temperature.			Aggregate of Mean Temperature.	Maximum.		Minimum.		Range.	Depth.	Rainy Days.
	Sun Rise.	Noon.	3 P. M.		Days.	Deg.	Days.	Deg.			
January,	29°	40°	43°	37°	14	60	31	7	53°	1.11	11
February,	30	42	42	38	25	66	1	4	62	2.13	9
March,	37	48	47	44	24	74	17	22	52	3.82	14
April,	46	54	55	52	18	75	13	27	48	2.72	9
May,	58	82	84	75	15	91	10	48	43	0.15	5
June,	65	78	80	74	2	94	6	54	40	3.83	15
July,	66	81	83	76	13	90	25	56	34	3.48	5
August,	66	80	79	75	7	86	8	60	26	2.45	14
September,	58	73	74	68	4	88	30	44	44	1.37	12
October,	49	63	63	58	5	78	27	30	48	5.81	9
November,	37	49	48	45	7	64	19	30	34	1.85	9
December,	30	40	39	36	17	60	27	4	56	1.53	5
										30.25	117

The fractions of mean temperature are omitted, to guard against confusion.

The number of days on which rain or snow fell during the year 1824, was 105. In 1825, 89 days—depth of water, 31.50 inches.

N. B. The snow was melted, and is included in the above statement of rain.

MEDICAL SCHOOL OF MAINE.

The Medical Lectures at Bowdoin College, will commence on Tuesday, the 20th day of February, 1827.

Theory and Practice of Physic by DANIEL OLIVER, M. D. Professor of the same department at Hanover, N. H.

Anatomy and Surgery by J. D. WELLS, M. D.

Midwifery by J. M'KEAN, M. D.

Chemistry and Materia Medica by P. CLEAVELAND, M. D.

The *Anatomical Cabinet* is very valuable and extensive.

The *Library* is one of the best Medical Libraries in New England; and is every year enriched by new works, both foreign and domestic.

Every person becoming a member of this Institution, is required to present satisfactory evidence, that he possesses a good moral character.

Citizens of Maine in indigent circumstances may have *surgical operations* performed, free of expense, if brought into the vicinity of the College during the Course.—As a reduction in the price of boarding is an object of importance to many, arrangements have been made, which, it is hoped, may effect this object to a considerable extent.

Brunswick, September 26, 1826.

MEDICAL LECTURES.

The Medical Lectures in Brown University, R. I., will be commenced on the third Thursday in February, 1827, and be continued about three months. Tickets to all the Lectures—$ 40.

WELLS & LILLY

Have just published Good's Book of Nature, in 2 vols. 8vo.

ERRATA FOR LAST NUMBER.

FURNISHED BY DR. LUCAS.

Page 242, for *how those causes*, read now those causes.
246, line from the bottom, strike out the words, and in different diseases.
249, for *I apprehend this to be the fact for that state of the liver*, read I apprehend this to be the fact from that state of the liver.
250, *have gone home*, read have gone hence.
of the same class of cases were those twelve, read of the same class of cases were those healed.
incorrect pathological view of them, read incorrect pathological view of it.
251, *strong mucous*, read tough mucous.
252, *merely corroding*, read thereby corroding.
253, *the base of the liver becomes smaller*, read the base of the liver becomes larger.
255, *the head appressed*, read the heart oppressed.
the utility of a cure by incision, read inutility of a cure.
257, *or best elevated*, strike out, best.
258, *these accumulations*, read those accumulations.
259, *as well in their relations*, read in their operations.
262, *will depend*, read should depend.
is to do nothing, read was to do nothing.

EXTRA-LIMITS.

WITH the present number, we commence an increase in the number of our pages.—Under this department we propose to give such matter as we may consider interesting, and which does not come immediately under the other departments of the Journal.

The first article according to this new arrangement, is a letter addressed to the Hon. Isaac Parker, by Professor Warren, of Boston; in which is given, not only an account of a most interesting and professional trial, but many valuable practical remarks on dislocation of the hip joint.

The candid and honourable manner in which Dr. Warren treats the subject, led us to believe, that if he could be prevailed on to give us permission to republish the letter, it would be pleasing to our patrons; accordingly, Dr. Warren's consent was asked, and generously granted.

A

LETTER

TO THE

HON. ISAAC PARKER,

CHIEF JUSTICE OF THE SUPREME COURT OF THE STATE OF MASSACHUSETTS,

CONTAINING REMARKS

ON THE

DISLOCATION OF THE HIP JOINT,

OCCASIONED BY THE PUBLICATION OF A TRIAL WHICH TOOK PLACE AT MACHIAS, IN THE STATE OF MAINE, JUNE, 1824.

BY JOHN C. WARREN, M. D.

Professor of Anatomy and Surgery in Harvard University, and Acting Surgeon in the Massachusetts General Hospital.

WITH AN APPENDIX of Documents from the trial necessary to illustrate the history of the case.

TO THE

HONOURABLE CHIEF JUSTICE PARKER.

Dear Sir,

It may be thought strange by some persons, that I should address to you a pamphlet on a subject appertaining to the medical profession. This liberty I have been led to take, partly from having intimated to you in conversation, that my opinion had been misrepresented in the publication which has drawn forth these sheets; and partly, from having considered this topic to be connected with jurisprudence, as well as medicine: but principally, that I might add respectability to these remarks by the sanction of a name, high in official station, and still higher in public confidence and respect.

The circumstances which involved me in this trial, and compelled me to make this publication, were briefly these. An individual, who had met with a grievous injury, applied to me for professional aid. This was administered to him to the utmost of my ability. He went away, and I considered my concern in the case to be closed. Much to my surprise, and I may say, dissatisfaction, after the lapse of a considerable time, I was summoned, in a legal way, to give a deposition to the facts of this case, within my cognizance. Being thus compelled, I gave the deposition, to the best of my memory, and with the most impartial disposition as to the parties. At the distance of two or three years, more or less, for I do not remember exactly, it came to my knowledge, that this same deposition had been made use of in a court of justice; that having been thought to operate unfavourably on one of the parties, it was criticised by the attorney for that party, and a very great error discovered therein; and that the said attorney had commented on my opinion and practice in very severe and overpowering terms. My colleague physicians in the Massachusetts General Hospital, though less concerned in the business, were, I was told, also attacked, and made the objects of sharp remarks. Even public institutions, it was said, from the unfortunate circumstance of my connexion with them, had not altogether escaped the *tirade* of the said attorney. A great triumph, I learnt, had been thought to be obtained; and as the sphere of a court of justice was thought too limited for enjoying it, an account of the whole affair would soon be given in print, by the ingenious attorney, and would be distributed widely through the country, to the great confusion and serious detriment of my unfortunate, mistaken self and colleagues.

Not long after, I saw and read the publication. Though formidable at first view, a very little examination was sufficient to satisfy me, that it was one of those things, which might be safely trusted to the salutary reaction of public opinion; and that I had nothing to do, but to lay it aside and forget it, which I accordingly did.

At a subsequent period I was informed, that the confident tone of the adverse plea in this case had really imposed on many persons not well acquainted with the subject; that my silence on an affair of so much eclat, had been construed into an admission of the charges against me: and that if I really had any thing to say in my own defence, it was proper to say it, for the sake at least of my colleagues, and that of public institutions, even if I was indifferent to my own reputation.

Such being the opinion of estimable persons, I determined to attend to the business, as soon as I could find a convenient opportunity; and if, on due investigation, it should appear that I could by any means poise the weighty arguments of the attorney, and muster facts able to stand up before those arrayed against me, to do it with the least possible delay. Since then, I have waited for the convenient time; but finding that unhappily it did not arrive, it became imperious on me to take the field, under all disadvantages, and to make the most of the resources ready at hand.

There are people, perhaps, who may think it hard, that the fact of my having been engaged in performing an official duty, should expose me to be the mark and butt of a witty attorney, first in a court of justice, and then in print. To this complaint it would no doubt be answered, that the cause of public justice required such an exhibition, and that a good citizen should submit to these sacrifices of feeling, without a murmur. Whether, if such answer were made, it would be thought a true and honest one, I submit to the decision of the public; and especially to that most able portion of it, the profession of the law. They can best tell, whether right and justice are helped by declamatory harangues respecting private character; and whether, when these have been delivered to full effect, in the court of justice, it is necessary, judicious, and worthy to be a precedent, to put them in print, and distribute them through the country for general information. If such a course is considered justifiable, it surely becomes me to warn my own fraternity to be careful of giving depositions for legal purposes. For there is, perhaps, no one secure against the hazard of being made a subject of similar attack, and its necessary consequences, the loss of time and relinquishment of duties: and to whomever it may so happen, he must even be willing to think himself fortunate, if he should have the lot to fall into the hands of an equally ingenious, witty, and well-bred attorney, who, while he delicately cuts off part of his reputation, is willing to allow him the remainder.

The publication of this trial, however, may not be without its advantages; for, as in the course of the discussion I have been compelled to state the principles on which dislocations are produced and cured, it may thence happen that some gentlemen, reading this is as a controversial pamphlet, which perhaps they would have passed over, had it been a formal dissertation, may come to the opinion that dislocations are to be known and treated by fixed rules, formed on scientific bases, and not to be considered the subjects of a practice altogether empirical or inheritable by nature.

In settling the course best to be pursued, in an affair made difficult by the number and acuteness of my antagonists, the decision of one point has given me much concern. The opinions of medical practitioners, some of them very respectable, were brought forward in opposition to mine, long after mine had been known; a part of these, indeed, for the most manifest purpose of overturning the notions I had ventured to form. It was indispensable for me to meet these contradicting opinions, to examine their strength, and to show, if I could, their weakness. In doing this in print, I perceived there was danger of coming in contact with the feelings of the parties concerned, and throwing on them some degree of the inconveniences to which I had been exposed. This is what I seriously wished to avoid. I wish to wound no man, however culpable; and especially I desire not to couple the name of any one, in a permanent bond, with the imputation of error. This evil has, I trust, been avoided by the omission of all names, to which I was obliged to fix terms different from those of commendation. Offensive epithets, indeed, I have sought to avoid wholly; and if in any case I have failed, it will be a subject of greater regret to me, than to any other person.

The defects of style, the want of logical arrangement, and the interminable use of the first person singular, require, I am well satisfied, a great

many excuses; and I am well aware, that none such are ever tolerated. For the last, perhaps, I ought to offer the necessity of the case; but as to the former, I can only plead, in abatement of censure, the very irregular, unsettled nature of the profession I have the honour to cultivate; and the serious obstacles it presents to pursuing a continuous train of thought. All know that the high responsibilities of this profession demand a great quantity and a great variety of personal experience in books, and a thoughtful application of the whole to the new cases of each day: but there are few people who know, or at least who realize, how little the medical practitioner of this country can command his time and thoughts, and how freely these are at the disposal of every body besides himself. Such considerations will, I am sensible, form an imperfect excuse for defects of style in this pamphlet; but happily, style, force of epithets, and even logical arrangement, are comparatively not important; my main business is with FACTS.

Boston, July 4, 1826.

LETTER.

SOME TIME in the month of December, 1821, I was called to visit a stranger at a public house in Boston. He informed me that he had received a severe injury in the hip, and had travelled a considerable distance for the purpose of obtaining surgical aid. His account of his misfortune was as follows: He was riding a spirited horse, when the animal, being restiff, suddenly reared, and fell backward on his rider. The weight of the horse was received on the inside of the left thigh, the patient having fallen on his back, a little inclined, as he said, to the left side. When he attempted to rise, he found himself crippled, and had continued so from that time.

I proceeded to examine the nature of this injury. For this purpose, the patient was stripped, and placed in different situations. First, I examined him in the upright posture,—afterwards sitting, and then lying on a bed. I made various movements of the disordered limb, and compared it with the sound one. Having continued this investigation as long as my time permitted, I told the patient, Mr. LOWELL, that his case appeared to be peculiar and difficult; that I must examine him again, and in the meanwhile should consider the appearances. He readily assented, and it was agreed that I should see him the next day. As the case was uncommon, I felt myself bound to consult the best authority on the subject. For this purpose, I referred to the late work of Sir Astley Cooper on dislocations, and with the additional light thus obtained, proceeded the next day to another investigation of the case. The patient was again examined with care and attention. The result was a conviction that the limb was dislocated. The appearances were such as I thought could not be attributed to any other kind of derangement. The two most remarkable of these, omitting others at present, were that the thigh was bent forward, so as to form a considerable angle with the body; and, at the same time, the limb was longer than the other by about three inches; that is, the knee of the injured side extended about this length, lower than the other. Such an appearance as the last named, it was clear, could only be caused by a displacement of the head of the thigh bone, or a very extensive fracture of the bones of the pelvis. As the other distinguishing symptoms of the latter accident did not exist, it followed, that the deformity must arise from the former. The determination of the direction taken by the head of the bone when it was

thrown out, or the kind of dislocation, was not so easily settled. As the limb was lengthened, it was clear that the present situation of the head of the bone could be neither above, nor on a level with, but below that of its natural socket; or that the dislocation must be downwards. There being two courses which the head of the thigh bone could take in a downward direction, either forward or backward, I had next to consider with which of these two accidents the phenomena would best coincide. If the dislocation were forwards, I might expect to find the round head of the thigh bone in front of the socket, and near the upper part of the thigh; the limb being at the same time much turned out; on the other hand, if the dislocation were backwards, the head of the bone would be found behind and below the socket, and the knee of the patient, according to Sir Astley Cooper, as I thought, and also according to some other authors, turned in towards the other. The existence of the former of these signs, of the dislocation backwards, and the most striking and characteristic one, I ascertained to my satisfaction; but the situation of the limb did not accord with the representation given of its necessary direction in such a case. What decision could I come to in this state of the contradictory phenomena? On the one side, there appeared to be the opinion of a high authority; on the other, that of my own senses. When I further considered the matter, it became obvious that the symptoms mentioned by Sir Astley Cooper referred to a different accident; to a lodgment of the head of the bone higher up; and that if a lodgment of the bone should take place on the parts below the level of the socket, the movements of the limb would not be confined to the same degree, as in the other instance, nor would it have the same direction. Having duly considered this, and not being able to find the head of the bone in front of the articulating cavity; but on the contrary, being satisfied of its existence behind it, I concluded that the dislocation was backwards, as well as downwards. The more full and continued display of the disordered part, and of its movements during the subsequent attempts to reduce the bone, by giving additional means of examination, confirmed me in this opinion. Such, or nearly such, was the course of my observations and reasonings, at the time when the injury was presented for my opinion.

Having formed a judgment of the nature of the derangement, I told the patient, that I was sorry to be obliged to say, that he had a dislocation of the hip joint of the most difficult kind. This information did not seem, as I had expected it would, to surprise him; for having heard my opinion, he proceeded to state, that he had in the beginning supposed the limb to be put out; that he had applied to surgical practitioners in his vicinity, and had been operated on for a dislocated hip; that the bone was supposed to have been restored to its place; and that he himself thought so, till he got up and began to try his limb, when he found it in a crippled state, and was after a time led to believe, that it had never been reduced to its proper situation. He did not, as well as I can remember, name the practitioners to whom he had applied, nor did I hear their names till a long time after, nor know their situations till I saw the published trial.

A conversation then took place between Mr. Lowell and myself, which was as follows. "Since my limb, as you tell me, is out of place, what is to be done to restore it?" I answered, "nothing." "Cannot you restore the use of my limb?" "No." "Are not limbs sometimes set after they have been out of joint as long as mine?" "Sometimes; instances of the shoulder being set, after having been dislocated a number of months, have occurred, but I never knew a thigh-bone restored, after it had been dislocated so long as yours." "Are you not willing to make an attempt to get the bone into its socket? Must I remain a cripple for life?" "Dislocations of the thigh bone are usually difficult to reduce, even when they are recent. Yours is the most difficult of four kinds of dislocation; con-

sidering this fact, and the time elapsed since its occurrence, there is no probability that your limb can be restored; notwithstanding, if you wish a trial to be made, and are prepared to submit to the suffering connected with it, I am ready to make it in the best manner I can for you." "Would it put my life in danger?" "I think not; I should not feel warranted in carrying any measures so far as to expose your life." "Well, I am willing to suffer any thing to get the use of my limb. I am young, and depend on my bodily exertions for my living. I am ready to run some risk of my life, for the chance of getting the use of my limbs again."

As we could not expect to find in a public coffee-house, the accommodations and apparatus necessary for our purpose, I advised Mr. Lowell to go to the Massachusetts General Hospital, where every thing requisite would be amply supplied. In order that he and I might have the full advantage of the surgical skill of the place, I requested a meeting of the consulting physicians of the Hospital. Four, out of six, of these gentlemen attended —Drs. Townsend, Mann, Welsh, and Spooner. To these gentlemen I stated in a very general manner, that the patient appeared to have a dislocation of the hip, which had existed some time; that he was desirous to have an attempt made to reduce it, although I had discouraged it. I then proposed that they should examine it themselves, and give their opinion of the nature of the accident, and of the expedience of an attempt to restore the limb to its place. The patient was accordingly subjected to their examination. They conducted it in their own way, and continued it, I presume, till they were satisfied; but what was their mode of examination, and what the time taken for making it, is wholly out of my recollection. This however is certain, that they came to a decided conclusion, that Mr. Lowell's limb was displaced; and as to its restoration, they were equally well satisfied that it was highly improbable, if not impossible. They thought, however, that if the patient well understood, that its reduction could not, upon any common and reasonable principle, be expected, and that he would experience much pain in the attempt, and that if, with this knowledge, he still continued to desire the operation, it would be expedient to perform it. The ground on which this conclusion was placed, was not the wish of the patient merely; but it was founded on the conviction, that he must be maimed for life, if left in the state in which he then was; on the consideration, that though the successful result of an attempt at reduction was highly improbable, it could not be considered as an absolute and physical impossibility; and on the maxim, that a doubtful remedy in a desperate case is better than none. I desired the gentlemen to converse with the patient, to state to him their opinion, and hear what he had to say. This they did; and, as I was absent during a great part of the time occupied by this conversation, I cannot say what passed; but after I rejoined them, I heard them use language not calculated to induce him to submit to an operation; and I heard him repeat to them, very much the same words he had before used to me. Such being the result of this conversation, and such the resolution of the patient, I felt encouraged to employ my best exertions for his relief from his present unfortunate condition.

Arrangements were therefore made, to prepare him for the operation, according to the practice founded on the doctrines of the great master of his art, the sagacious John Hunter; which have been so ably displayed by his pupils, especially the distinguished Sir Astley Cooper.

"In attempting," says Mr. Hunter, "the reduction of a dislocated bone, the indications are,

"1. To make the naturally most immoveable part a fixed point.

"2. To overcome the action of the muscles, which draw up the dislocated bone, and resist its recovering its natural situation.

"3. Then by making lateral pressure, to force the head of the dislocated bone into its place.

"Here then a retrograde motion is to be observed, and the last action in the dislocation is the first to be overcome; the last action is that of the muscles drawing up the dislocated bone, and their force being overcome by distension in a proper direction, and by it, the head of the bone being brought to the edge of its receiving articular surface, is then by lateral pressure to be forced into its place.

"If a dislocated bone remain in its unnatural state a considerable time, and in contact with a bony surface, by degrees it forms a new socket for itself; the bone against which it presses undergoes absorption, and the adhesive inflammation arising in the parts around, a new joint is formed. These may be called necessitous joints.

"This happens commonly in irreduceable dislocations of the thigh-bone."

The principles roughly sketched by John Hunter, have been more distinctly displayed by Sir Astley Cooper. While Mr. Hunter taught that the principal resistance to reduction was caused by muscular action, Sir Astley Cooper has told us how to overcome this resistance. Instead of attacking the resisting powers by mere mechanical force, he goes at once to the source of these powers, and cuts it off. The means he suggests for effecting this object, are all those agents known to weaken the muscles. Of which, bleeding, the warm bath, and nauseating medicines, are among the most efficient.

Guided principally by such authorities, I proceeded to make the arrangements for the proposed trial. The patient was about thirty years of age, I presume, and rather muscular; but having been recently confined by his accident, the muscles were not rigid. In the morning, he was subjected to the action of a powerful purgative. The warm bath being afterwards prepared, he was immersed therein, at as high a temperature as he could bear, and retained there for an hour. Small doses of tartar-emetic were given, in the way suggested by Sir Astley Cooper, so as to produce that deadly sickness, which relaxes the whole muscular system: finally, at the moment the apparatus was to be applied, a vein was opened in the arm, and blood drawn as rapidly, and in as large quantities, as the faintness of the patient permitted.

The apparatus having been previously prepared, he was placed on a table covered with blankets, on his right side, and carefully fixed to the table by bands passed round the pelvis. Other bands were passed between the thighs, and secured to the wall of the apartment. Thus the pelvis, being immoveable in the transverse and in the longitudinal directions, was made the "fixed point," on which to operate. The powers designed to extend the limb were then applied in the two directions opposite to those of the forces meant to keep the body steady. Soft substances were placed around the middle of the thigh, and about them a strong band, to which were attached the dislocation pullies, in such a manner as to draw at right angles to the body; while other cords were fixed above the knee, to draw the limb downwards in a direction opposed to the cords passed between the thighs. These last were of course so situated, the patient being on his side, that the extension downwards might be forwards, or backwards, and were entrusted to the hands of as many individuals as were required.*

* At first view it might be thought unnecessary to extend, or lengthen, a limb already too long; but as we shall again state hereafter, when a bone is thrown out of its socket, some of the muscles are overstrained, and draw the limb towards them, until it hitches or is engaged somewhere; and these muscles confine it in its new situation, till their contraction is overpowered. The object of the application of force in a longitudinal direction, in such cases, is not to make the limb permanently longer, but to disengage it, so that it may afterwards be drawn or pushed into its natural place; a change, that would be produced in this case, by the resisting muscles themselves, provided the bone were previously disengaged.

The necessary articles being arranged, the longitudinal force was put in operation, by the hands of the assistants, and the lateral force by the pullies, both of them in the most gradual manner, so as not to excite the muscles to contract by sudden jerks; but by a steady, long continued force, gradually to exhaust their energy, and overcome their resistance to reduction. While this was doing, I occasionally examined the situation of the head of the thigh bone, placed my hand thereon, and felt its movements, produced by the extending forces. I also requested some one or more of the consulting physicians to feel the head of the bone, with the intention of ascertaining whether its motions were such as to encourage us with the hope of its being approximated to its socket; and they did one or more thus examine and recognise the movement of this part, with an expression of their opinion that the change of position was not considerable, nor sufficient to lead us to believe that the bone would be reduced. When the extension had been made for some time, I seized the extremity of the limb, and operating with it as a lever, while the lateral force was still acting, made strong movements of the limb, with the intention of breaking it from its situation, by destroying its newly formed adhesions of cellular membrane. Aware that dislocations had been often reduced, in a manner the most opposed to that expected, even after judicious and scientific means had been tried in vain, I was studious that no practicable mode of operating should be omitted. The direction given to the limb was therefore varied in every possible manner; and this practice I adopted with more decision, from reflecting that if the limb were not restored to its place, yet the contraction of the muscles would be partially overcome by this process, the motions of the limb subsequently increased, and the condition of the patient improved. Such was the course adopted during this operation. I often questioned the patient as to the degree of suffering, and, on his declining any complaint, increased the application of force. After this had been continued an hour or more, with such changes as circumstances suggested, the patient appeared to be satisfied that the reduction was impracticable, and agreed to a discontinuance of the operation. The consulting physicians, being also of opinion that every warrantable effort had been made, advised to forbear any further attempt. At the time the force was about to be removed, I put in practice a recommendation, made, I think, by Sir Astley Cooper, for a dislocation backwards, and which I had employed in a successful manner in a former case, of iliac dislocation; namely, to pass a round towel, called by us a roller, between the thighs of the patient, to throw it over my shoulders, and then make an effort to raise the upper part of the bone, while the foot was carried inwards to the other. This affords a mode of operating in a very powerful manner. I found it not more successful than the other means employed, and therefore desisted from further trials. Thus terminated the operation, in the manner I had expected and represented.

Although the means necessarily employed to give the patient a full experiment appeared to me severe, he submitted to them most courageously, and never uttered a complaint, that I know of, at that time or since. In a day or two he was well enough to leave the hospital.

From a publication made by him, after the last trial of his cause, I learn that he was so little appalled by the severity of the operation, and had suffered so little injury from it, that after an interval of a single week, he was able and willing to apply to another practitioner of surgery, of much experience and skill, and to submit to another effort at reduction. This second trial having terminated like the first, he was induced, after the interval of a fortnight, to apply to a third operator, a person of reputation for his success in reducing dislocations: and he even a third time underwent the process of extension. The result of this attempt was the same as that of the first and second. The efforts made in the two last attempts

were, it is said, at least as forcible as those made in either of the preceding.

By one acquainted with these facts, it would scarcely be credited, that the operation at the hospital was represented by the attorney for the defendant, in language calculated to excite the indignation of a jury, and to give them a strong prejudice against those who could advise and practise such cruel proceedings.*

Some time after these occurrences, I met the patient, Mr. Lowell, in the street, and was accosted by him. He informed me that he was about to return home; that he had thoughts of instituting a suit against the physician and surgeon, who were first called to him, and that he should be glad to have from me a statement of his case. On hearing of this intention, I immediately remonstrated with Mr. Lowell. I told him that he ought not to blame his medical attendants, for his case was one of difficulty; that they had done the best they could for him; and that if he trusted any medical man, the responsibility must rest with himself, for, if he had not confidence, he should not have employed the individual. To this he replied, that he would not desire to bring an action against the surgeon, had he done the best he could; but that he thought himself grossly neglected; that when he first suspected his limb had not been reduced, as it was thought to be, he sent for the surgeon, and was not able to obtain his attendance till a number of days afterwards; that, when he did come, he acknowledged things were not right, but left him without making any attempt to rectify the difficulty; and that he was, in consequence, a cripple for life. He thought he was entitled to compensation for the injury he had suffered in being thus neglected. My answer was, that there were probably circumstances connected with this apparent neglect, not fully understood, and which would give a different aspect to the case; that certainly I was of opinion he could not expect to recover damages in such a case, and therefore strongly advised and hoped he would lay aside all thoughts of a lawsuit, and must excuse myself from giving a statement, or in any manner interfering further in the affair. With this we parted, and I believed that what I had said to him, would influence him to abandon the project of a prosecution.†

* Vide Appendix, No. III.

† This conversation, apparently trivial, I have stated as minutely as I can remember it, because a story has been industriously propagated in Maine and in Boston, that not only was the lawsuit, instituted in this case, founded on the opinion I gave of the injury, but moreover, that I encouraged and stimulated the patient to seek reparation for his misfortune in this way.—Nothing can be more untrue. What possible motive could I have, for harassing an unfortunate practitioner with a lawsuit? It is insinuated that I wished to gain some little eclat by contrasting my discernment with the ignorance of a remote practioner, and that I expected to bring into notice the Massachusetts General Hospital, by the display of an attempt to reduce an irreducible dislocation. As to the latter insinuation, it requires no answer; and for the former I must, though with diffidence, assert, that I have always been desirous of supporting and aiding individuals, and contributing my humble aid to improve the medical profession. Let it not be forgotten, however, that our first duties are to the public. It is for them we exist, and to them we are answerable. No one should be deterred, by the apprehension of giving offence, from a conscientious expression of his opinion, when it is really required. Dr. Hosack, in an inaugural address, has very justly remarked, that "Coalitions, except when they are directed to mutual improvement, or the benefit of the community at large, are inconsistent with, and a violation of, the solemn obligations which the physician enters into, when he receives the Doctorate of Medicine." There are instances when the declaration of a mistake on the part of a medical practitioner could be productive of no possible benefit to the patient, and might do harm to the physician. Under such circumstances, silence would not only be proper, but necessary. It must be a miserable mind that would devote to ruin the reputation and happiness of another, merely for the gratification of his own pride. Such individuals are sometimes seen; but happily they are few, and reap their reward in the frowns and aversion of all good and high-minded men.

Although I have been led by these reports to advert to the topic, yet it is not, in fact,

Some months after the conversation with Mr. Lowell, I received a letter from the practitioner chiefly concerned in this case, couched in terms apparently friendly, containing a request that I would give my opinion of the nature of this accident, and of the treatment he had adopted. The writer was till this time unknown to me, even by name. Not being aware of the object of this application, and believing that he wished to clear himself from the effect of reports which might have been circulated in regard to the affair, I wrote a hasty and brief answer, and without changing the expression of the opinions I had already given, I said what I conscientiously could to relieve his mind, and to give him some means of showing to others, that I was not disposed, by my view of the case, to promulgate opinions more unfavourable to his practice than I was compelled to do by a regard to truth.* This letter, written hastily, as a private communication, without any suspicion on my part of the existence of a legal process, was subsequently annexed to a deposition, which I was called on to give, and produced in court, and though it was objected to by the court, as not admissible, yet the attorney for the defendant has thought proper to print it in connexion with the trial.

Perhaps the terms in which I endeavoured to frame an excuse for the practitioner, may be thought to little qualified; my apology must be found in a sincere desire to prevent his suffering evil consequences, from what I then considered his misfortune, rather than his fault.

So long a time elapsed after the letter, before I heard of this affair, that I did not expect to be troubled on the subject again. This ignorance on my part arose, as it appeared afterwards, from the distance by which the parties and myself were separated from each other, a space of three hundred miles. At the expiration of about a year, more or less, from the occurrence of the accident, I received a letter from the patient, Mr. Lowell, in which he informed me that he had recently been examined by a practitioner of surgery, of much experience and ability, who had declared in the most unequivocal language, that his hip was *not out of joint, nor ever had been so.* Mr. Lowell appeared to consider this opinion lightly. To me it appeared so extraordinary, that a practitioner, coming many months after others, who had examined the accident in the fullest manner, and with the most impartial intentions, should form an opinion directly opposite to the former, that I was incredulous on the subject. That another surgeon should differ from me concerning the kind of dislocation, considering the infrequency of the species, would have excited no surprise; but the general symptoms of displacement had appeared to me as strongly characterized as those of any dislocation, of any joint, which had ever presented itself for my examination; other persons had been equally well satisfied, and this at an early period of the derangement; so that it may well be thought I was a little surprised at so unexpected a declaration.

A suit, it appears, was instituted about this time, by the patient, against the surgeon and physician first called to administer to him. The nature and grounds of the charge made against these gentlemen, may be gathered from the testimony in the Appendix.† It seems that the medical practitioner first called, examined the injury, pronounced it to be a dislocation of the hip joint, made attempts to reduce it, and did, as he thought, succeed; but the friends of the patient not being satisfied, another, more in the habit of surgical practice, was called from a neighbouring town, and took the management of the case. He examined the disordered limb, declared it not

important to the subject of these remarks. The opinion I gave on this injury, was not meant to correct the error of another person. As I have already said, I had not a suspicion, when I formed it, that any physician or surgeon had been consulted in the case. My attention was fixed on the character of the injury, and the possibility of finding a remedy. Knowing no reason for concealment, I told the patient my opinion frankly and fully.

* See Appendix, No. V. † Vide Appendix, No. I.

to be reduced, and proceeded to take measures for effecting its reduction. After a short time, he considered his efforts to have succeeded, and the limb to have been reduced. The patient was put into bed, with his knees tied together, and left in the care of the physician first called, who visited him daily; while the surgeon, living at a distance, and on the other side of the water, saw him occasionally. After some time, the patient, finding his limb lengthened, and without its natural power of motion, became uneasy, and desired the attendance and examination of the surgeon. The latter, on his visit, seems to have declared himself not well satisfied with the appearance of the limb, but did not then, or at any subsequent time, propose any important measures for reducing or improving it.

On the first trial of this action, the jury gave a verdict for the plaintiff, and awarded $1900 damages. On a second trial, the verdict was again for the plaintiff, damages $100. On a third, of which a report has been printed, the jury were not able to agree, except in acquitting the physician first called; and, one of the jurors being unable to attend longer, on account of ill health, the parties agreed to withdraw, without costs on either side. The result of this last trial was, therefore, in fact, favourable to the defendant.

It would have been better, perhaps, if the affair had thus terminated. Unluckily, the attorney for the defendant, probably with his sanction, or at his request, thought fit to prepare and publish a report of this trial, with his argument at full length, containing statements and representations calculated to produce unfavourable and erroneous impressions in regard to the professional conduct of my colleagues and myself, and to bring ridicule on the institutions, with which we are connected. In the investigation of a case of local injury like this, it would seem quite a flight of imagination to reach even the character of a hospital; but the writer has brought within his compass, the medical school and University of this vicinity. Productions like this, involving the reputation of individuals, whether professional or moral, are among those most eagerly read; hence this "Report" has, with the aid of a little industry, been widely circulated. Its representations have had some influence on the public mind, and given some uneasiness to those concerned in the welfare of these institutions.*

* The ingenuity displayed by the attorney for the defendant in this trial, was certainly very meritorious; but it does not appear to have been necessary to blazon it by a publication of this description. Especially, when the display was to be made at the expense of professional men, against whom there could be no crime charged,—unless it were one to perform a charitable act, in the course of their official duty, and afterwards to state their knowledge of the fact, in obedience to the call of public justice. The attorney for the defendant has in his advertisement said, "There can be no doubt of the respect entertained for the eminent talents and virtues of those distinguished individuals, whose opinions are commented on, or controverted." If this general remark were sincere, why did he think it necessary to put the names of these individuals in print, in connexion with terms of ridicule; and to send them thus throughout the nation, and, as far as he is able, to posterity. A person of discernment could not but be aware of the probable operation of such a pamphlet, in the possession of the evil spirits of society. The writer has indeed assigned, as a justification of the proceeding, that a partial account of this case had been recently published by the plaintiff, and that it was consequently necessary to prepare one more complete and just. The account published by the plaintiff was in truth little more than an expression of his feelings, under the influence of recent disappointment. Having in himself the full conviction of the reality of an injury, of which the existence had been ridiculed in a court of justice, he printed a pamphlet in the form of an "Appeal" to the public, in order to convince them of his true condition, and to relieve himself from that state of excitement, produced by the final and irremediable failure of hopes which he had long been accustomed to cherish. The publication on the other side, is of a very different character. It is not the work of one of the parties concerned, but that of the attorney, who had already obtained all he had a right to ask for. It is a publication of some study and ability. Impartial men must undertake to judge whether a sudden display of feeling on the part of the plaintiff afforded a sufficient ground to justify the publication by the

Previously to the occurrence of this third trial, I was visited by the plaintiff, for the purpose of obtaining my deposition to the facts which had come within my knowledge. On learning the object of this visit, I declined any agency in his case. I stated to him, that all the service in my power had been rendered, and that I thought myself fairly entitled to be excused from further trouble, especially as I had discouraged the suit, and did not feel justified in taking any measures, likely to operate against the defendant; he being of the same profession with myself, and therefore entitled to a reasonable degree of fellow feeling. He replied, that he was an injured man, and must have justice, and that for this purpose my deposition was essential.

He left me without obtaining my consent to give the deposition, and soon after, a notification was received from Lemuel Shaw, Esq. to appear at his office, and answer to certain questions proposed by the parties on either side. I did not appear on this notice, but applied to Mr. Shaw, on whose science and integrity I could repose the most perfect confidence, with a view to ascertain whether I could be compelled to answer in such a case. This gentleman informed me that I must either appear to give testimony, or be responsible to the party aggrieved by my non-attendance, to the amount of damages by him sustained. A summons was afterwards issued, and money tendered in legal form, as is usual in cases where the attendance of a witness is to be compelled.

Accordingly I appeared, and submitted to a series of interrogatories and cross interrogatories, which occupied about three hours. My answers were made according to the best of my knowledge and recollection at the time; and comprehended an account of the facts of the case, already stated, with some others. The substance of these answers was nearly this: That I did at a certain time attempt to reduce the dislocated limb of the plaintiff, in conjunction with other physicians and surgeons named;—that the reasons for believing the existence of a dislocation in this case, were, the unnatural position of the limb; its elongation; the accompanying contraction of the thigh and leg; the displacement of the trochanter; the displacement of the head of the thigh bone, and its appearance in an unnatural situation, in or near the ischiatic notch; the inability to use the limb in the ordinary manner, and the consequent necessity of extraordinary and constrained movements;—that manual force might succeed sometimes, in reducing dislocations of the hip joint, and that when this failed, mechanical means must be employed;—that the elongation of a disordered limb might be produced, to some extent, by simple relaxation of the muscles, or by relaxation of the muscles accompanied with fracture of the bone;—further, that the thigh bone, after being reduced, might be thrown out again, by the application of considerable force, or by movements of considerable extent, but not otherwise;—that a surgeon who employs the best means in his power, ought not to be responsible in damages;—that a surgeon who has opportunity to examine a case of dislocation, immediately after the injury, other circumstances being equal, has better means of judging of the nature of the case, than one who examines several months after;—that the case in question, though not without its difficulties, appeared to me to be one, in regard to which, men of high standing in the profession would not

attorney for the defendant of a report, containing imputations, not only on the defeated and unfortunate plaintiff, but also on various professional persons, to whom he happened to apply for assistance. It would have been perhaps more judicious in this ingenious gentleman and his client to have been satisfied with success, and not have looked for revenge. They would then have spared me this disagreeable task, which nothing but the necessity of self defence would have compelled me to undertake. The statements made on the trial, which I have now been obliged to combat, might have stood well with the world always, for aught I should have done, had the defendant and his friends abstained from this and their other publications.

differ. The manner in which the reduction was attempted, was also described, though with less minuteness than as I have now described it.

The questions proposed to me were also proposed to three of the consulting physicians, who had examined the case with me. The coincidence of their opinions with mine, at the distance of more than two years from the occurrence described, affords a strong proof of the authenticity of the evidence, and the distinctness of the most important impressions made by this case on each of the individuals questioned.*

In the representation I gave of this affair, it is obvious that there was no disposition shown to bear more severely on the defendant than regard to truth demanded; that on the contrary, I was disposed to admit whatever might tend to afford him the fullest opportunity for self-defence. This course would, to many persons, seem to call for a corresponding demeanour on the part of those employed to defend the accused; but so far from this, the attorney for the defendant, not contented with the performance of his professional duty, that of exerting every effort to prove our conclusions to be groundless, has treated us in the manner considered allowable in courts of law, in the case of witnesses guilty of prevarication, malicious design, or gross falsehood. This course must be considered very objectionable, for the whole medical profession are liable to be called on for opinions, which would equally expose them to such treatment, if they happened to fall into the hands of persons inclined to practise it.

The attorney does not seem to be justified in his proceeding by an appearance of excessive confidence in the opinion I had occasion to express; although he has freely imputed such confidence. I stated, that this "dislocation was one difficult to discover, but one in regard to which *I thought* men of high standing in the profession could not differ." Could I have spoken less confidently in regard to any case, in which I had made up an opinion at all, especially after two several private examinations, confirmed by a third in public?

A specimen of the kind of language, to which I allude, is contained in the following extract from the argument of the attorney for the defendant. In this part of his address, there are not indeed any personal allusions of an extra-professional kind, but the quotation will exhibit the manner of the orator, and show whether he is authorized to charge others with too much confidence.

"Taking the account given by Dr. Warren of their reasons as the text—and reading their result by its own light, there needs no more satisfactory test for trying its own truth. Its condemnation shall come out of its own mouth; and on a critical examination it will not only prove to be a *perfect fallacy*—and shown to be a most *egregious error*, but demonstrated to be one of the most *absurd*, *unaccountable*, and *incredible* description. Its refutation can be established upon the most obvious *principles of anatomy*, which it will appear, have not only been disregarded by these *learned doc-*

* The attorney for the defendant has not neglected the opportunity of insinuating that there is something suspicious in this concurrence of testimony. "This circumstance is barely noted," he says, "at this moment, in conjunction with the *memorable coincidence* of opinion expressed by the learned faculty of Boston. In this view, however, it becomes quite edifying to observe the *exquisite harmony* prevailing in the sentiments of the other parts of the *choir*, swelling altogether beyond the *gentle cadence* of their *candid precentor*, that a dislocation of this nature was not altogether without its difficulty."

For this insinuation of a collusion between the other gentlemen and myself, there is as little ground as for other imputations thrown out in the course of the remarks of the attorney. The truth is, that I never saw the depositions of either of these gentlemen, and two of them certainly could not have seen mine; for their depositions appear to have been taken previously to it; nor has any other communication on the subject existed between us, from the day of the consultation in the Massachusetts General Hospital, in the month of December, 1821, to the moment when I am writing.

tors, but that their conclusion has been adopted in *absolute defiance* of them: a *hallucination* of the most extraordinary character itself—such as could hardly have been expected of a *surgeon of three months* standing."

To this language I shall reply by an investigation of facts.

In order to show what opinions the orator intended to convict of such perfect absurdity, it would be desirable to give his allegations and arguments in his own language; but such is the character of his harangue, as to render this impracticable. It may be gathered from it, that his object is to prove two allegations:

1st. That no dislocation existed in the case of Mr. Lowell.

2d. That such a dislocation as I supposed to exist in this case, is, according to the authority of Sir Astley Cooper, impossible.

By the aid of a deposition from a gentleman of high standing in the profession, showing that, in his opinion, no dislocation existed, and by presenting the authority of Sir Astley Cooper in a light favourable to his object, the attorney prevailed so far as to obtain a disagreement of the jury. Any jury must indeed have been greatly influenced by the consideration, that one surgeon of eminence doubted the existence of a dislocation; for a doubt from such a quarter would alone seem sufficient to exonerate the defendant from the charge of ignorance, or at least partially so.

The charge of negligence, the ONLY GROUND, as it seems to me, of accusation, that ought to be maintained in such a case, does not appear to have been much insisted on, by the attorney for the plaintiff, at least if we are to judge by this "Report."

My design, it will be perceived, is not to prove the criminality, or inability, of the defendant. With his practice I wish to have nothing to do. The object I have in view is to defend myself against the allegations mentioned above, and against the means employed to support them. I shall, therefore, proceed to examine them successively: and,

First, whether a dislocation did actually exist in the present case.

The appearances which led me to believe, that there was such a dislocation, I have already stated. It is therefore proper to enquire, what are the grounds on which the opinion, thus given, has been proved or supposed to be erroneous; and they appear to be constituted by the depositions of four medical men.

It seems that about six months after I had seen this case, and nine months from the occurrence of the accident, the plaintiff was examined by a surgical practitioner of much reputation and experience, who pronounced that no dislocation did then exist, and he moreover intimated that none ever had existed.* Another practitioner was present at the examination, who agreed to this opinion. After a period of two years or more, when the cause was for the third time on trial, two other gentlemen appeared in court, neither of whom, so far as we learn, had examined the patient, yet both of them made oath, that the accident was not a dislocation, but a fracture, or *some other* injury of the joint.

The deposition first alluded to, which will be designated as deposition A., being first in point of time, comes first to be considered. In commenting on this deposition, proceeding as it does from a gentleman of high standing in the surgical art, I feel some degree of embarrassment and reluctance; yet, whoever considers the matter a little, will, I presume, see that I have been compelled to it by the publication of this trial, and the gross insinuations it contains. The gentleman in question examined the case six months after my colleagues and myself, gave an opinion wholly opposite to ours, and one tending to show we were in the wrong in every point. I do not say that he did so with this intention, but his opinion had this effect, and influenced others to take the same ground; it was brought into court,

* See the deposition marked A. in the Appendix.

served as an important instrument in the hands of the attorney, and finally was employed to assist in making a book, of which the obvious tendency is to injure our professional character. I hope therefore to escape the charge of a want of due respect, to a gentleman in advance of me in age and experience, if I examine the ground taken against me very fully, so long as the examination is conducted in a decorous and honourable manner; and I trust that in doing this, I shall make use of no expression, which is not called for by the defensive nature of my position.

It is to be considered, that the circumstances attending the accident of the plaintiff, were such as were likely to produce an injury of an important character, such as fracture of the bones of the thigh, or pelvis, dislocation of the former, &c. Several kinds of injury were *prima facie* to be regarded as probable consequences of the fall. The subsequent phenomena showed that one or more of these actually existed, and every medical man, of many who examined the patient within a period of nine months from the date of the accident, had decided that the actual result was a dislocation of the thigh bone. The defendant indeed supposed this to be complicated with fracture of the socket, but all agreed in asserting its existence. The deponent A, however, was induced, by the result of an examination, made at the end of the above mentioned period, to conclude that all his predecessors had been mistaken, and that the injury was of a different kind. In order to have an adequate sense of the importance of this deposition, it is further to be considered, that the question had become unusually important, and that the decision of the court concerning it was a matter of extensive interest, and likely to affect in various ways the property and character of many individuals. In the testimony of a distinguished person, given under such circumstances, we should expect a clear statement of the phenomena, which he considered inconsistent with the existence of dislocation, and of those, which in his opinion tended to prove that of some other possible derangement. We shall be able to estimate the force of the reasoning on these points better by reducing the substance of the testimony to distinct propositions. These are,

1st. That the manner in which the accident happened, was such as to render it scarcely possible that the hip should be dislocated.

2d. That some of the phenomena, as the lengthening of the limb, distortion, &c. which had been attributed in this case to dislocation, did in other instances, and might in this, arise from other causes.

3d. That the symptoms exhibited in Lowell's case differ in some essential circumstances, from those which are exhibited by any one of three different species of hip dislocation.

Which of these propositions points out any circumstance of the plaintiff's accident, that in the opinion of the deponent is incompatible with the notion of a dislocation of the hip bone? The first alone. The argument is, that, since Mr. Lowell fell on his hip, the fall could not dislocate it. But Mr. Lowell informed me and others, that he fell on his back, and Reynolds, the only witness of the circumstances of the fall, swears positively to the same effect.* The deponent, therefore, being misinformed in regard to a fact, his opinion, so far as it rests solely on this fact, is without weight.

The second proposition points out nothing inconsistent with dislocation. The phenomena detailed belong to dislocation. They belong also, according to the deponent, to other derangements. I shall hereafter endeavour to make it appear, that, in the manner and degree in which they existed in Lowell's case, they could not be accounted for, on the supposition of any other cause than dislocation. I might safely admit, however, for the present, that they could, for this would prove only that Lowell's injury *might be* fracture, hip disease, or something else, not that it *was not* dislocation.

* See the deposition of Reynolds. Appendix, No. II.

The remaining proposition goes to prove merely, that the bone in this case could not have been dislocated in any one of three different directions mentioned by the deponent. It will hereafter be seen that I agree with him in this opinion. Still there is nothing decisive against the existence of dislocation, unless it be asserted, that it is possible in no other direction, which the deponent nowhere says.

The whole of this part of the evidence of the deponent is evidently connected with the first proposition. Conceiving it impossible that a dislocation should result from a fall on the hip, he was naturally led to seek for some other probable explanation of the phenomena. This singular difference of opinion is therefore to be traced to incorrect information.

This is equally obvious when we consider the evidence, contained in the deposition, in another point of view, that is, as it sets forth the circumstances, which, in the opinion of the deponent, are decisive of the existence of a particular species of injury. The only one offered is a sort of corollary from the first of the above propositions. "A fall on the hip," &c. says the deposition, "might drive the head of the bone through the socket," &c. and "in my opinion, if there is any derangement of the bones, it is a fracture." The "fall on the hip," again, is the only decisive circumstance in favour of fracture.

It is not necessary for me to point out the incorrectness of the deponent's inference from the supposed fact. It is not expected of any one to draw correct conclusions from false premises, and it must be obvious to every reader, that the deponent was under the necessity of coming to an erroneous conclusion.

My situation as defendant in this affair, obliges me further to remark upon two other opinions which appear in this deposition. They have not indeed a direct bearing upon the present question of the existence of *any* dislocation. But they are likely to have an influence upon the judgment of the public in regard to it, since, if correct, they prove that the opposite opinions, previously expressed by me, are unfounded, and thereby go directly to diminish the confidence in my testimony. I refer to the deponent's opinion concerning the length of time after the accident, which may render it difficult or impossible to restore a dislocated limb, and to the opinion concerning the use of pullies:

"I should not think," says the deponent, "that a hip joint having been out of place *six* or even *eight* weeks, would render it impossible to reduce it. It might even be *a more favourable time* for the operation, than immediately after the accident, especially if the soft parts were much bruised and swollen." This doctrine appears new to me, I must confess, and will probably be so to most surgeons. It has been generally thought to be of primary consequence, to effect the reduction of a dislocated bone with the least possible delay; because, independently of all reasoning, *experience has taught*, that the difficulty is less during the first hour, than during the second, and so on; and that a dislocation which might be reduced with ease the first day, would present a great resistance the next. No practical axiom has been more generally adopted, therefore, and none more firmly established, than that which teaches the necessity of immediate surgical aid in such injuries; and what was before established by experience, has been confirmed by the discovery of the principles which operate in producing dislocations, and which ought to govern the attempts to remedy them. To the school of Hunter we are indebted, I believe, for the more distinct knowledge of the fact, that the great obstacle to the restoration of a dislocated bone is to be found in the resistance of the muscles. In the passage already quoted, Mr. Hunter lays down as a principal indication in the reduction of dislocations, that we are "to overcome the muscles, which draw up the dislocated bone, and resist its recovering its natural situation."

The old opinion that ligaments only prevented the bone from returning into

its socket, is generally abandoned, as it has been well ascertained, that there are but few instances where they prevent a bone from obeying any external impulse after these accidents. In a complete dislocation of a capsular joint, such as the shoulder or hip, the ligament is in most cases extensively torn; a part of it usually remains connected with the bones, and this part helps to retain the bone in its new situation, though not sufficient to do it of itself. In joints, confined only by lateral ligaments, as the fingers, a dislocation may take place without a rupture of the ligaments, and, when this happens, the resistance will be made by these, and not by muscles; whence it has been proposed to divide one of these ligaments, in the case of an irreducible dislocation.

Having made these remarks to explain how far the action of the ligaments appears to be admitted as a cause of resistance, it may be repeated, that the doctrine generally received at the present day is, that the muscles are the principal agents in producing and maintaining dislocations. In causing dislocations, by a well intended effort of nature, they act in a violent and unexpected manner; using the bone as a lever, and an exterior body as a fulcrum, so that the bone is displaced more by their action, than by the external violence. In retaining the bone in its new place, they are governed by the disposition, common to every healthy muscle, to shorten itself as much as possible.* Whenever a bone is thrown from its socket, the situation of most of the muscles about it is altered; some of them are lengthened and others shortened. The latter immediately begin to contract, in order to accommodate themselves to their new situation; and as soon as they have attained the degree of contraction their state permits, their relations to the joint are changed, they are no longer in a condition accommodated to its ordinary structure, and oppose a return of the bone to its place. The muscles, on the contrary, whose two points of attachment are removed further from each other, are elongated and overstrained to such a degree, that they would immediately draw the bone back to its proper place, were it not hitched on some bone or muscle; but this being the case, their contraction, instead of restoring the bone, does indeed confine it in its unnatural situation, and tends to prevent its being disengaged. In the dislocation of the thigh bone downwards, the Psoas magnus, Iliacus internus, and part of the Gluteus medius are put on the strain. It appears that on the occurrence of a dislocation, the bone is confined at first by the *overstrained* muscles only, as, in the instance named, by the Psoas, Iliacus, and Gluteus; but after the other muscles have had time to contract, these also add their powers to that of the first, and incalculably increase the resistance. There is another way in which muscular action may be concerned. It is when the bone, in its passage from the socket, perforates a muscle, or forces its way between two muscles, and the muscular fibres contract about and embrace it. This is probably the case in many of those instances, in which, after the unsuccessful exertion of a powerful force upon a displaced bone for a considerable time, a slight movement effects its sudden restoration, by accidentally bringing the head of the bone to the rent in the muscles. Sudden reductions, I imagine, often happen in this manner; though they may arise from simply disengaging one bone from another, and sometimes, but more rarely, from the bone having been confined by its ligament.

A knowledge of the principles alluded to, will show the vast importance of an immediate reduction in most cases of displaced bones. The practice ought not to be lightly abandoned, because the soft parts are swelled. The swel-

* When the knee pan is broken, the muscles connected with the upper fragment of the broken bone, draw it upwards several inches; their action in this, is analogous to that of a spiral spring, drawn out and then abandoned to its elastic power. The same occurs in the rupture of the Tendo achillis near the heel; the upper fragment of the muscle contracts, and leaves an interstice between the divided parts. The laws governing the muscles in such instances, exert their influence in dislocations.

ling of the soft parts at first, exists principally in the cellular substance, which can oppose no considerable obstacle to the movements of a bone; but, if the operation be delayed, the muscles also may swell, and add a new and insuperable difficulty to the reduction.*

Sir Astley Cooper relates indeed a very extraordinary instance, at first view opposed to what has been just said, of an individual who put his hip in accidentally, after it had been out a number of years. This case does not, however, afford much encouragement for delaying the operation; for the reduction had been unsuccessfully attempted by a number of eminent surgeons. Such extraordinary events ought to have no influence in opposition to the maxims drawn from the experience of every day; and I earnestly hope that practitioners will not risk their reputation, and the happiness of their patients, by an hour's delay in these cases. In regard to hip dislocations, I must say there is the strongest reason to believe, that the intervention of a few days between the accident and the operation will form insuperable obstacles to the reduction of such dislocations in the greater number of cases.

In the attempt made at the Massachusetts General Hospital to reduce this dislocation, it was thought proper to employ the dislocation pullies provided for such purposes. The deponent takes occasion to speak of their use in the following terms. "I do not think that the mechanical powers, such as the wheel and axle, or the pullies, are necessary to reduce a dislocated hip, or any other dislocation. They have been sometimes used with effect, but they have oftener been injurious; and what can be effected with them, can be effected without them. It is not the quantum of force which reduces dislocated bones, so much as it is the direction of the force, and this can be given by the hand of skill better than by pullies," &c. These remarks would be perfectly just, if the principal opposition to reducing a dislocation, was made by the ligaments of the injured joint; but, as we have already said, it is now generally understood by surgeons, that the greatest obstacle is muscular contraction. The power exerted by the muscles in such circumstances, is known to be very great, and requires a great force to counteract it. When a bone is displaced, some of the muscles, as we have already shown, are shortened, and contract to accommodate themselves to their new condition; others are put on the stretch, in consequence of the hitching of the bone in an unnatural situation. The last description of mus-

* As a wrong opinion on a practical point of so much consequence might have an unhappy influence on the fate of many persons, and the reputation of many young practitioners, I shall beg leave to introduce from Sir Astley Cooper some remarks on this subject, for the benefit of those who may not have access to his treatise.

"Of the various accidents which happen to the body, there are few which require a more prompt assistance, or which more directly endanger the reputation of a surgeon, than cases of dislocation. If much time elapse before the attempt at reduction be made, the difficulty of accomplishing it is proportionably increased, and it is not unfrequently totally impracticable; and, if the nature of the injury remain unknown, and the luxation consequently unreduced, the patient becomes a living memorial of the surgeon's ignorance, or inattention. In a dislocation of the os femoris, which still remains unreduced, a consultation was held on the nature of the injury, and after a long consideration, this report was made by one of the surgeons, 'Well, sir, thank Heaven, we are all agreed that there is no dislocation!'—I am of opinion, that three months, for the shoulder, and eight weeks for the hip, may be fixed as the period from the accident at which it would be imprudent to make the attempt at reduction, except in persons of extremely relaxed fibres, or of advanced age. At the same time, I am fully persuaded, that the shoulder has been reduced at a more distant period from the accident than that mentioned, but in most instances with the results I have been deprecating.

"In very strong muscular persons, I am not disposed, after three months, to make the attempt, finding that the use of the limb is not, when reduced, greater than that which it would have acquired, by having remained in its dislocated state. *Let this be fairly represented to the patient*, and then, *at his request only*, the reduction should be attempted."

cles are, immediately after the accident, the most efficient causes opposed to the restoration of the bone. The first object to be considered in the attempt to bring the bone to its place, is the application of a power in such a direction as to overcome these muscles, not by sudden, violent efforts, but by a gradual force. When they are strong, this application of force can be more effectually made by pullies, than by the hand, for the muscular resistance is so great, in such cases, as not to be overcome but by pullies, or the hands of many men. The power of a number of persons cannot be exerted steadily, their strength is soon exhausted, and the degree of force applied must, therefore, constantly vary; the muscles of the patient, instead of being overpowered, are stimulated to contract with additional force, by this irregular action. Pullies, on the contrary, act steadily and uniformly, until the muscles, on which they draw, are perfectly extended. The degree of power applied, may be tolerably well measured by the observation of the surgeon, and increased or diminished, as may be necessary. Here is no interference of a multitude of assistants with each other; no bustle, no unnecessary violence. The direction of the force may be so adapted, as to act on the muscles that resist, and not on others. The only advantage possessed by manual force, which pullies have not, consists in the facility of varying the direction of the powers, so as not only to overcome the muscles whose action is visible, but that of others, which is not so obvious; and moreover, by these changes to succeed in bringing the head of the bone to the interstice of the muscle, or to the rent in the ligament, through which it may have passed. These advantages may be acquired, however, in an easier way, by overpowering the muscles by pullies first, and afterwards, if necessary, varying the direction of the limb by the hand. These instruments are therefore of great use in many simple dislocations, and, in such as are extraordinary and difficult, they are indispensable. I am well satisfied that many bad cases of dislocation of the hip can never be reduced without their aid. As to injurious consequences arising from them, I must say, that, though every powerful instrument may be abused, I have never seen any ill effects from pullies, nor do I perceive that any such can occur, without great inattention.* Sir Astley Cooper, and most surgeons of the present day, very strongly recommend their use. The wheel and axle I have not heard of as being employed in modern days.†

Besides that of the deposition A, which we have been examining, it has been observed, that there was also the negative evidence of three other

* It must be understood, that exception should be made for cases, the circumstances of which might justify a great and even dangerous application of force.

† There is one other point in the deposition, to which I am induced to advert, from unwillingness to allow a stigma to attach to the members of the medical profession in this country, which I cannot think merited.

Question, by the defendant's attorney. "Is the dislocation of the hip joint an unusual occurrence? and might a skilful surgeon fail in any attempt to reduce it?"

Answer. "A dislocation of the hip joint is very rare, and probably *not one medical man in ten* would be able to reduce it."

Lamentable indeed must be the condition of surgical science in our New England states, if only one practitioner in ten is able to reduce a dislocation of the hip. Can we boast no better fruits from our six medical schools, from all our state medical societies, and our district medical associations? The mode described by the deponent with so much candour, as having been employed by himself, does not appear to be a process very difficult to imitate. "I once," says the deposition, "reduced a hip joint; it was dislocated upwards and backwards; and after pulling it in every direction but the right, it was reduced easily by carrying the knee towards the patient's face. I had the assistance of two men only." This operation might be practised easily by persons of less surgical knowledge. If the medical men of this and the neighbouring states were questioned, I imagine they would tell us, that not one in ten would fail in reducing a dislocation of the hip; making due exceptions for cases of peculiar difficulty, such as those of the ischiatic notch, and those, in which the accident was overlooked or mistaken for a different kind of injury.

practitioners. That of one appears in the form of a deposition; that of the others in the form of testimony in the court of justice. The weight to be attached to these will be the next object of our investigation. Of the three it appears that only one did ever actually examine, or rather see the examination of the patient, and, of course, the opinions of the others are of comparatively small importance. I shall first consider the evidence of the former, contained in the deposition B.*

From this it appears, that the deponent B. was present with the deponent A., "at an examination of Charles Lowell, for a *disease*, or affection of the left hip joint," and he says, "I am of opinion, that it does not arise from dislocation at present existing; but from affection of the muscles, or *some other cause*." That is, the deponent was *present* at an examination in which a most strange mistake made by a number of surgeons, was detected and exposed; and yet, so far as we can discover, did not put his finger to the affected part. For there is nothing in his deposition which would lead us to believe that he did more than stand by and witness the examination made by another person. Was it not natural for the deponent A., on making the discovery that the imagined dislocation did not exist, to express himself strongly to the deponent B., and to request him to examine for himself; to point out to him the exact situation of the different parts concerned, and desire him to touch them, that there might be no mistake? and would it not be expected that B., in his turn, should have dropped some expressions of surprise? and would he not have repeated these expressions in his deposition? We hear nothing of this.† The deponent A. made his examination, drew a number of lines about the joint, while the deponent B. remained merely *present*, and seems to have felt very little interest, in a most extraordinary occurrence. How are we to understand all this? The deponent B. it is intimated in the pamphlet of the plaintiff, had been a pupil of A., and consequently, perhaps, submitted wholly to his judgment. If this be the fact, we cannot be surprised at the posture he assumed in the affair. But, while it may be said he was right in yielding his judgment to that of a person of superior experience, we must be excused for not admitting that this opinion adds any weight to that of the former. There is another point of view, however, in which we cannot so fully exonerate the deponent; for, although he does not inform us that he examined the nature of the injury, yet, it appears, he was able to get an opinion concerning it, not of a very positive character indeed; which he thus expresses, "That it (the disease or affection) does not arise from dislocation at present existing, but from affection of the muscles, or *some other cause*." Does not this language amount nearly to saying, that, though he could not pretend to state what this disease, or affection, was, he was willing to believe that it might arise from an affection of the muscles, or from *some other cause*, but was determined not to believe the existence of dislocation.

The testimony of the witness C.,‡ I shall not criticise very minutely, as the circumstances under which it was given, and the fact of his never having examined the patient, render it of little consequence. "From the nature of this testimony," says the attorney on the same side, "it is not capable of being perfectly reported." The difficulty of reporting it is explained in the pamphlet of the plaintiff, who tells us, that both this practitioner and the witness D., were said to be threatened with a similar prosecution if this succeeded, and that, from this or some other cause, the witness laboured under great agitation. For this feeling he is not blameable; but what shall we say to the testimony of a witness, who never saw

* See, in Appendix, deposition B.

† See testimony of Theodore Lincoln, annexed to deposition A, in Appendix.

‡ See Appendix, C.

the object of whose existence he would testify. He ventures to give an opinion, "that the plaintiff's injury was a *fracture*, not a *derangement* of the bones of the pelvis." Yet this fracture, and derangement, he had never seen, nor had he any precise means, that we hear of, of judging of their existence. The same remark may be applied to the testimony D,* which informs us, that the plaintiff's case "was an injury of the bones of the pelvis." He had no better opportunity of ascertaining the real state of things, than the witness C. Neither of them, as is positively stated by the plaintiff, ever examined him.

It appears that of all the medical men who did examine him, not one doubted that his hip was dislocated, except the deponents A. and B., both of whom saw the patient nine months after the accident. We have already seen that these two are reducible to one; and that the deposition A. gave rise to all the others. Thus we have the imposing testimony of four medical practitioners, really reduced to that of one individual, who, as we have seen, was, in this particular instance, misinformed in regard to the circumstance upon which his opinion was principally grounded.

The examination of these depositions and testimonies, will satisfy most men, probably, that while an inclination is displayed to make use of every fact and circumstance, to combat the opinions rendered by my colleagues and myself, yet the arguments adduced in supporting the adverse doctrines have no solidity. If this be true, I might here leave the question of the existence of the dislocation, to be decided by the impartial; and I should be contented so to do, were it not necessary to use every honourable means to repel so gross an imputation as that of subjecting an individual to a cruel operation, from an ignorant or hasty investigation of his case. I cannot therefore properly omit any thing which may tend to present the true state of this affair in a stronger light, and to clear myself from a charge so odious.

Besides those already employed, there seem to be two other tests, to which this question may be subjected. The first is, the general agreement of the symptoms exhibited in this patient, with the signs of dislocation, and their want of agreement with those of any other injury of the hip. The second will be found in a comparison of the weight of opinion on the opposite sides of the question.

In many cases of dislocation, the changes in the external appearance of the joint are such as to enable us to infer with certainty the displacement of a bone, though they may not show at first view, in what direction it has happened. This is especially true in regard to the hip joint. At present, therefore, it may be best to examine the general phenomena of these dislocations, without attempting to settle the characteristics of any species. The latter must be made a distinct and special subject of consideration.

In studying the external appearances indicative of the displacement of a bone, a kind of knowledge is required, which is not to be found in books. It may be called the anatomy of the superficies of the body, and consists in an acquaintance with the appearances made on the surface of the body, by bones, muscles, and other parts. Our students consider it essential to qualify themselves thoroughly in the science of the interior arrangement of these organs; yet, when called for the first time to the examination of an injured joint, they find that the knowledge, of which they had fancied themselves possessed, has vanished from their sight, and left them an obscure and confused group of appearances, among which they cannot with certainty discriminate a single object. For many years I have been in the habit of impressing this subject on the minds of those gentlemen, whom I have had the honour to instruct; and though I am not certain, that it has gained the attention which its importance merits, I am confident that these

* See Appendix D.

lessons have not been without their influence in practice. By having pointed out to them the prominences of the joints in different parts of the body, they learn to observe with more care the external appearances, accustom themselves to the impressions made on the senses of vision, and touch, and thus become prepared to notice the changes in the aspect and relations of all these parts, when disturbed or displaced by accident or disease. The condition of an articulation is sometimes so strangely altered by injuries, that the most scientific eye, and the most practised hand, seek in vain to ascertain with precision, the changes which have taken place. But this obscurity may be cleared away in most cases; for with sufficient patience and careful observation, the anatomist will rarely fail of satisfying himself of the real state of the injured parts. The advantages of the kind of information noticed above, I have been accustomed to enforce by referring to the skill of those who, practising without a knowledge of anatomy, and therefore constantly liable to do wrong, yet often succeed in doing right, where the most scientific surgeons have failed.

This kind of knowledge of external parts is absolutely necessary in trying the question now at issue; for, as we cannot see what the interior state of the organs is, in a dislocation, we must judge of it by external signs. These signs may be supposed to be fallacious; and this is true in regard to a part of them; while others are to be considered as carrying with them much the same certainty as ocular inspection of the bones and muscles. To obtain a statement of these symptoms, we must have recourse to works creditable not for their scientific character only. They must also be popular works; that is, such as contain facts considered to be admitted by the medical world. As one, which combines these qualities, I shall select the work of Mr. Samuel Cooper. In the part relating to the subject we are upon, he gives us a translation from Boyer, so far interwoven with his own opinions, as to afford us the advantage of the doctrines of the able French surgeon, admitted and adopted into a standard English work.

"The general symptoms termed by Boyer *positive*, or actually present, are, says Mr. Cooper, numerous and clear." We shall state them, in order that they may be compared with those of our patient.

"1. In dislocations of orbicular joints," (as the shoulder and hip) "and complete luxations of ginglymoid joints," (as the elbow and knee) "the orbicular surfaces are not at all in contact, and the point where the dislocated bone is lodged, cannot be on the same level with the centre of the cavity from which it has been forced. *Hence a change in the length of the limb.*

"2. In almost all complete luxations, the *direction of the axis* of the limb is unavoidably altered.

"3. The absolute immobility of the limb, or at least the *inability of performing* certain motions, is among the most characteristic symptoms of dislocation.

"4. In dislocations with elongation of the displaced limb, the general and uniform tension of all the muscles arranged along it, gives to these organs an appearance, as if they lay nearer the circumference of the bone, and the limb were *smaller* than its fellow. On the contrary, in dislocations where the limb is shortened, the muscles are relaxed, but, being irritated, they contract, and accommodate themselves to the shortened state of the limb. Hence the extraordinary swelling of the fleshy part," &c.

"5. The circumference of the joint itself presents alterations of form well deserving attention, and, in order to judge correctly of these symptoms, correct anatomical knowledge is highly important. When the head of a bone articulated by enarthrosis" (a joint with a ball and socket) "has slipped out of the cavity, instead of the plumpness, which previously indicated the natural relaxation of parts, the *head of the dislocated bone may be distinguished at some surrounding point* of the articulation, while, at the

articulation itself, may be remarked a *flatness*, caused by one of the neighbouring muscles stretched over the articular cavity, and, more deeply, may be perceived, the outline and depression, produced by the cavity itself."*

Such are the symptoms which Boyer and Mr. Cooper consider as *positive* signs of dislocation. Let us compare them with the appearances in our patient's case.

The first is "a change in the length of the limb," because "the articular surfaces are not at all in contact." This phenomenon was remarkable indeed in our patient; the limb being *longer than natural*, from two to three inches. Had it been shortened, it might have been alleged, that the neck of the bone was broken, or that the socket was fractured, or that the muscles were contracted; but it seems incredible, that an elongation so great, taken with the accompanying appearances, can be attributed to any cause, except a change of relation between the head of the bone and its socket, owing to the articular surfaces being "not at all in contact."

The second change is in "the direction of the axis of the limb." This

* As it is desirable to place the general signs of dislocation in the most distinct point of view, I shall beg leave to make a quotation from the writings of a very able surgeon, and anatomist, Mr. Wilson. In his late lectures before the Royal College of Surgeons in London, we find the following account of these symptoms.

"Luxations are productive of some symptoms, which are peculiar to the nature of the accidents, but common to every joint affected by it; and of others, which must belong to the particular joints.

"Some of the attending symptoms are not of a distinguishing nature, but are common to luxations, fractures, and even violent contusions. Among the last are the deformity and altered shape of the limb, and the pain felt, which is increased by any attempt at motion.

"It is evident, that no luxation can take place, without some change in the form of the joint. In most cases, part of the dislocated bone will be felt, forming a prominence in a new situation, while a vacancy will be perceived in the part from whence it was driven. But these alterations cannot always be detected, from the quantity of blood that may be immediately diffused in and around the joint, or from the swelling and tension, that may have subsequently taken place. If a cylindrical bone is luxated, the extremity farthest from the accident, not partaking of the swelling, will, from its position, and the general direction of the bone, inform us of the position and situation of the dislocated parts; and this will be known more accurately, from the naturally perceptible projections of the bone, either not being found in their usual places, or from their being felt in other parts.

"A change, in general, will be produced in the length, as well as the direction of the limb, and the usual motions of the joint will not take place, even when attempted by the hands of the surgeon. And should the joint have possessed rotatory motion, this will be completely lost. It is not so in a fracture of the bone; for motion may, at first, be produced by the surgeon in every direction, although crepitus, and a considerable degree of pain will attend the attempt. Indeed, immediately on luxation happening, if it is complete, the bone will admit of some motion, although this in a very short time is prevented, by the contraction of the muscles, which keeps the bone rigidly fixed in its new situation; but all rotatory motion is generally lost from the first. I have, however, in a dislocation of the shoulder joint, some hours after the accident, found so much motion, that, had not the head of the os brachii been felt in the axilla, and a vacancy been perceptible under the acromion scapulæ, the nature of the accident might have been readily mistaken.

"A sensation not very dissimilar to crepitus will sometimes be perceived, in attempting to bend the joints of a dislocated limb, before much inflammation has come on, and after it has subsided, which has led to the supposition of the accident being a fracture.

"When the extremity of a bone has been completely thrust from its socket by violence, the joint having been, previous to this, free from disease, the capsular ligament must always be torn.

"Cases, however, of incomplete luxation may happen, in which both sets of ligaments, although stretched, shall remain free from laceration.

"In the reduction of a luxation, the removed bone must first be brought to a level with the edge of its socket, and then by lateral pressure, or rotatory motion, returned to its cavity.

"The difficulties that attend the reduction of a luxation, will be much increased by the delay, which may have taken place between the time of the accident's happening, and the attempt to remedy it."

symptom was even more obvious than the former, the limb being permanently projected in front of the body, so as to form a considerable angle with it. The axis of the limb did not correspond with the axis of the body as it should do, nearly. Its direction was such, that a line continued in the direction of the thigh from the knee forwards, would have struck the ground at some distance in front of the body. There was therefore a very conspicuous alteration "in the direction of the axis of the limb."

Third, "the absolute immobility of the limb," "or at least the inability of performing certain movements." "Absolute immobility" in this instance, there was not indeed, but a decided "inability to perform certain movements," there was; all the motions of the limb were diminished; the power of extending or carrying it backwards, and the motions of flexion, abduction, and adduction, were impaired; that of rotation hardly existed. On the whole, the degree of inability, though not so great as in some dislocations, was such as impartial persons would consider to correspond as nearly as might be, with the period of the accident, and the situation of the bone below its socket, on a part, which could not closely confine its motions. The unfortunate patient felt deeply that his locomotive powers were materially circumscribed, and his "inability of performing certain motions," was a source of great unhappiness to him.

Fourth, "In dislocations, with elongation of the displaced limb," all the muscles have a general and uniform tension. They appear hard as if tightly stretched. On the contrary, when the limb is shortened the contracted muscles swell out. There was none of the latter appearance, and the former was as remarkable as it could be, at the end of three months, during which all the muscles had time to accommodate themselves, in some measure, to their unnatural situation; and here I remark that the axioms of Boyer relate to signs presenting themselves immediately on the occurrence of a dislocation. The lapse of time will diminish the prominence of all the symptoms, of some, to a great, and of others, to a less degree.

Fifth, "The circumference of the joint itself presents alterations of form." The hip in our case was elongated, and the hollow caused by the falling of the trochanter below the socket, was so remarkable that it was noticed by the patient himself, alarmed him, and led him to say to the surgeon, "Dr., explain to me the cause of the hollow on the outside of the injured hip?" "The outline and depression of the cavity" we could not expect to discover in the *hip-joint*, after three months dislocation, though we might in the shoulder. But "the head of the dislocated bone might be distinguished" with due care and attention below and behind its natural cavity.

Thus we find all the positive marks of dislocation existing in this case. Their agreement with the symptoms laid down by the best writers is in every respect as exact as it possibly could be, at the period of our examination. Will any impartial man remain in doubt as to the nature of this accident?

The symptoms of the plaintiff having been shown to correspond with those of a dislocated limb, it may be well to continue the comparison, in order to show that they do not correspond with any other alleged injury of this part.

From these depositions and testimonies it would appear that it is intended to say, that the existence of certain other injuries than dislocation, would explain the phenomena of this case in a more satisfactory way than the supposition of a dislocation; and that the injuries thus supposed, or alleged to exist, are, 1st, Fracture of the bones of the pelvis. 2d, Contraction and relaxation of muscles, similar to what takes place in certain voluntary motions, or in the hip disease. 3d, An injury of which the precise nature is obscure. Let us then proceed to compare the symptoms of these

injuries successively, with the appearances exhibited in the case of the plaintiff.

1. Fracture of the bones of the pelvis.—

The consequences of a fracture of the socket, or any extensive fracture of the pelvis, especially such as could allow the thigh bone to hang down two or three inches, must be exceedingly formidable, and even in a large proportion of cases, fatal. In order to produce such a phenomenon, the pelvis must be crushed in pieces, I should conjecture, for I can only conjecture, since there is not, that I know of, any account of a fracture of the socket, accompanied with material elongation of the limb. On the contrary, in all these accidents, the limb is *shortened*, if its length is in any way altered. Let us resort to the authority of Sir Astley Cooper on this point.

He gives us an account of three such cases only. Two are minutely described, the other generally. The first was that of a man, on whom fell a hogshead of sugar. The injured limb was two inches *shorter* than the other. There was a distinct grating of the bones of the pelvis; the knee and foot were turned inwards; the patient died the same evening. The second had the appearance of a dislocation backwards; the symptoms are not related; the patient died in four days. The third, very minutely described, was that of a woman, who had been crushed between a cart and lamp-post. She lived in a very distressed state from the 8th to the 24th of the month. Sir Astley also informs us that he has known three instances of recovery from simple fracture of the os innominatum. In neither of these was the socket in the slightest degree involved, nor does Sir Astley mention any instances of fracture into the socket, which did not prove fatal.

We may infer, from what is stated by this author, that a fracture of the socket is an accident followed by most violent and dangerous symptoms; and that they are wholly dissimilar to those of the case of our patient. This will more fully appear from his general account of the symptoms of these fractures. "As these accidents are liable to be mistaken for dislocations, and as *any extension* made for them adds extremely to the patient's sufferings, and is liable to produce fatal consequences, even if there was previously a possibility of revovery, I am anxious to say a few words upon them.

"When a fracture of the os innominatum happens through the acetabulum, the head of the bone is drawn upwards, and the trochanter somewhat forwards, so that the leg is shortened, and the knee and foot are turned inwards. Such a case then may readily be mistaken for dislocation into the ischiatic notch. If the os innominatum is disjointed from the sacrum, and the pubes and ischium are broken, the limb is a *slight degree shorter* than the other; but in this case the knee and foot are not turned inwards, but outwards. Of the first of these accidents I have seen two examples, of the latter only one. The accidents are generally to be detected by a crepitus being perceived on the motion of the thigh, if the hand be placed on the crista of the ilium; and they are attended with more motion than occurs in dislocations."*

Let us compare the appearances laid down as indications of a fracture of the socket spoken of in the depositions, with the appearances presented in the disordered limb of the plaintiff.

* By the remark that these accidents might be mistaken for dislocation in the ischiatic notch, Sir Astley means the dislocation backwards, and not that backwards and downwards; for he expressly states the limb to be shortened in every instance of these fractures, whereas in the dislocation downwards it is lengthened. Sir Astley lays down symptoms quite adequate to enable us to distinguish a fracture of the socket from a dislocation. These are a crepitus, or a grating of the fractured pieces on each other; a greater motion of the limb; violent symptoms of inflammation about the joint, and finally, very distressing effects on the whole body, and even a fatal termination.

(1.) In his case the limb was *lengthened;* in fracture of the socket it is *shortened.*

(2.) In him the axis of the limb was changed; in fracture of the socket it is not changed.

(3.) In him the power of moving the limb was diminished; in fracture of the socket, it is increased.

(4.) In the plaintiff, the muscles were elongated and tense; in fracture of the socket, they are shortened and relaxed.

(5) The form of the hip is changed in both cases, but in a dislocation downwards, it is longer and narrower, and was so in him; whereas in fracture of the socket, it is shorter and rounder.

(6.) In this case there was no grating of the fractured pieces of bone on each other, or *crepitus;* in fracture of the socket, this symptom is most remarkable.

(7.) The effects of this injury were not very violent and alarming, the patient was at no time in danger of losing his life; in fracture of the socket, on the contrary, the consequences are very severe, and, in the greater number of instances, even fatal.

Possibly it may be alleged, that I have assumed for the parallel the existence of the worst case of this injury, and that if, instead of it, I had taken one of the instances of fracture of the os innominatum, which terminated favourably, the resemblance between the two cases would have been greater. A little reflection only is required to see, that a fracture of the os ilium across its superior part, or a fracture of the parts of either of the other bones, out of the socket, would not for a moment be suspected as a cause of the phenomena in our case. To afford grounds for the supposition of elongation of the limb being caused by injury of the bones of the pelvis, this injury must not only be presumed to extend through the socket, but moreover there must be together with a fracture of the socket, a very extensive separation of the broken parts, a corresponding rupture of ligaments and an extensive laceration of the superjacent muscles. Such must be the state of things supposed to exist in an injury, which should be adequate to the explanation of the appearances in this case. I say, *supposed to exist,* for I am not informed that there is any such case on record; nor do I believe that such an occurrence as a fracture sufficient to give three inches increased length to a limb, could consist with the patient's life for a single hour.

From the comparison made above, it seems there is no great similarity between the symptoms of this case, and those consequent on any known, or supposable injury of the bones of the pelvis.

2. Contraction and relaxation of the muscles. The words are these: "As for the apparent lengthening of the affected limb, I think that is owing to the preternatural contraction and relaxation of the muscles situated about the hips; and is made to appear so by the twisting of the bones of the pelvis on the spine. Any person, when sitting on a chair, can by an exertion of the muscles make one knee project beyond the other, as much as Lowell's did when I saw him. The same lengthening of the limb takes place in a disease of the hip, called the hip disease, where no external violence has been received." There seems to be a slight incongruity between these two statements; for it is first said that the apparent lengthening of this limb is owing to the contraction and relaxation of the muscles; which we must suppose to have been brought on by the violence done to the part; but afterwards that the same lengthening may take place in a perfectly sound state of the bones; and in the disease of the hip, without external violence. To which of these causes is it intended to attribute this appearance? As the nature of the investigation in which we are engaged, compels me to consider, not only alleged, but also supposed cases, it becomes necessary to make three separate questions; and to examine, 1st,

whether the elongation and other phenomena were caused by a contraction and relaxation of muscles, consequent on violence; 2dly, whether this contraction and relaxation might not be a voluntary exertion; 3d, whether it might not be the result of a complaint similar to the hip disease.

First, a contraction of muscles is a frequent consequence of a wound, such as is made in puncturing a part with a nail, a needle, and sometimes a lancet; but as an effect of a bruise or contusion, it is a rare occurrence. A contraction brought on by a bruise, could take place only from extraordinary violence, contusing or breaking down the texture of the muscles, and thus producing inflammation, with its concomitant phenomena, and a subsequent rigidity from effusion of coagulated lymph, into the interstices of the muscular fibres.

In the case of the plaintiff the violence was not greater than we daily hear of from the overturning of stage-coaches and other carriages. In the second place, this violence was not inflicted directly on the hip, but on the back and thighs; and in the third place, the consequent symptoms were not such as to lead to any suspicion of severe muscular inflammation. We have not therefore any good reason for attributing the contraction to the direct effects of the accident. If we suppose a case of contraction actually following a violent injury, the train of events would proceed something in this way. The patient being thrown from his horse, falling directly on the hip, would, notwithstanding the great violence, find himself able to stand and walk at first. Soon after, the hip would swell and inflame, with much affection of the whole constitution. When the inflammatory symptoms had subsided, the muscles about the hip would be found contracted and indurated. The contraction would increase, and draw the limb more and more toward the body; soon the limb would waste away, and its power of motion gradually diminish, till the part became wholly, or in a great measure, useless. Such was not the history of our patient. Immediately on his falling he found himself maimed; his power of walking was at an end, the limb was at once distorted, and this to so great an extent, that he immediately desired surgical aid, to set it in order; afterwards he lay fifteen or sixteen days without extraordinary swelling, or other marks of inflammation. When he began to use the limb, its motions were limited, and difficult; but since they have been regularly increasing, as always happens in old dislocations from the formation of an artificial socket.

Let us in the next place, consider this contraction and relaxation of muscles, as an effect of voluntary exertion:

"Any person sitting in a chair, can, by an exertion of the muscles, make one knee project beyond the other, as much as Lowell's did when I saw him."

This remark is ingenious and true. When in the sitting posture, the bone of the opposite side may be made a pivot, whereon to turn the pelvis, which may be twisted and lowered, and the limb of the side protruded, by the aid of a constant and considerable effort. But, if the posture of the body be changed from sitting to upright, this kind of distortion vanishes, and it is not possible to continue the protrusion. Had such been the cause of the plaintiff's appearance, it would have been easy to have detected the fallacy, by examining him in different positions, and especially by causing him to walk, and thus rest the weight of the body on the disordered part. His difficulty continued the same, however, in all situations, and under all circumstances. Whether he was sitting, lying, standing, or walking; when his mind was occupied with other subjects; when he was submitted to the action of cords and pullies, and of manual force; when he was in his ordinary condition of health, as well as when reduced by the relaxing power of bleeding, and other debilitating causes;—under all these circumstances, the disordered limb remained much too long.

Lastly, we are to inquire, whether the supposed contraction and relaxa-

tion of muscles could be the effect of a complaint like the hip disease? That there is some degree of similarity between the appearances in this case, and those of the hip disease, must be admitted; but the resemblance fails in too many important and essential particulars, to allow us to confound the two with each other, or to authorize us to consider them as arising from an identical cause. The hip disease is preceded by a bad state of the constitution, and the first remarkable occurrence is a sort of limping gait, with a propensity to stumble, if the patient be a child, and an apparent weakness of the limb. This is followed by a pain in the knee, or rather in the thigh, a little above the knee; the limb wastes, becomes flaccid, and seems to lengthen a little. In moving the thigh bone in its socket, there is usually much pain. As the disease advances, the limb grows shorter, for it quits its socket, and is ultimately drawn up to the extent of a number of inches, the thigh being bent forward. The joint suppurates; matter is discharged through one or more apertures; and ultimately pieces of bone are separated, and expelled through the wounds.*

The history of this disease shows that it has not any similarity to that of the plaintiff, excepting in the elongation and flexion of the limb. The former symptom is slight, compared to what happened to him, and is only apparent,† since the lengthening actually arises from the descent of the pelvis on the lame side.

* It may be thought necessary to advance the authority of some author, to afford a more fair comparison of symptoms; and I therefore quote from Mr. Ford, who is considered a standard writer on this disease. "The caries of the hip joint, always painful in its consequences, and sometimes fatal in its event, steals on at first so imperceptibly, and arrests the attention so little, that it is scarcely considered as a serious disease. The constitution is not at first materially affected by it; paleness of countenance, fits of languor and heaviness, succeeded by a proportional flow of spirits, and sudden exertions, are accidents so common in the lives of young persons, that they pass for common circumstances, and attract but little attention. There are, however, symptoms often accompanying the first pathognomic marks of this disease, which are, a sort of limping gait, lameness scarcely discernible, and a weakness of the leg and thigh just beginning to be apparent.

"About this time, the limb, on strict examination, will be found, both in the thigh, and on the calf of the leg, to be really lessened in its circumference. A striking alteration likewise takes place with regard to its length. It is more or less elongated, the elongation is visible, and the degree of it ascertainable. Let the patient be placed in a horizontal position, with both his legs extended properly, for a comparison with each other; the patella, with the condyles of the thigh bone, will appear in the diseased limb, manifestly lower than the same bones on the sound side. It may be still farther ascertained by an inspection and comparison of the relative situations of the great trochanter, or the internal malleolar process of the tibia, with the corresponding parts of the sound limb. On viewing attentively the patient in an erect posture, it is clearly discernible, that there is on the diseased side, an alteration with respect to the natural fulness and convexity of the nates, that part appearing flattened, which is usually most prominent; and a little below, towards the thigh, there is a degree of swelling different from, and strongly contrasted to, the natural appearances of the sound limb."

† The cause of the fallacious appearance of elongation in the hip case, has not been understood till of late years, and even now does not seem to be generally adverted to by practitioners of surgery. Mr. Crowther, in his treatise on white swelling, having obtained the hint from Mr. John Hunter, first placed this matter in a distinct light. According to him there is no real lengthening of the limb in the hip case, but the apparent lengthening is owing to the inclination of the pelvis to the lame side, in consequence of which the limb descends lower than before; so that even when the patient is placed on his back, and in bed, the disordered limb seems longer than the other. The obliquity of the pelvis in this case, is, in the opinion of Mr. Crowther, to be ascribed to the patient's endeavouring to throw the weight of the body on the sound side. The cavity of the joint being diseased and tender, he is unwilling to rest the weight of the body on that side; and when he does so he does it with a bent limb, in order that the head of the thigh bone should not press so directly into the diseased socket. Formerly it was thought that the limb was pushed down by collections of fluids in the joint, by relaxation of the ligaments and other causes. If, however, the limb is measured from the joint to the heel, its length will be found to be the same as that of the other side.

In Mr. Lowell's case, the elongation of the limb was not only apparent but real; and in degree far exceeding what is known in any case of hip disease. The knee projected beyond the other, in a manner perfectly obvious, and to an extent by measurement, of about three inches. The concomitant circumstances were quite different from those of a diseased hip. He had no marks of inflammation, except, as we are told, for a few days after the accident, during which, the usual swelling and pain of a dislocation were seen to exist. When he recovered from its first effects, and from that time to the present, he has been as free from the signs of inflammation, as a man with a thigh bone out of its socket could well be. He had no pain nor distress on the limb being moved in any direction, no uneasiness on putting it to the ground. The pelvis was not more inclined than must happen in a dislocation downwards. The muscles put on the strain tend to approximate their two points of attachment; hence it must happen that in a dislocation downwards and backwards, the Psoas and Iliacus, the Pectineus, and especially the fore part of the Gluteus medius will pull the pelvis downwards, unless the head of the thigh bone is so wedged against it, as to prop it up. On the whole, it may be inferred, that there is little similarity in this to a scrophulous hip case, and, of course, the causes applied to explain the elongation of the limb in the latter, will not do it in the former.

The flexion of the limb considered alone, is not very important as a discriminating symptom, because it is found to accompany many disorders of the limb, yet we must notice that in few of them is the degree of contraction so great as in this dislocation.

In the remarks made above, the phraseology employed has not been adverted to, but it is now necessary to state that the terms in which this proposition is laid down, are objectionable. Namely, that the apparent lengthening of the affected limb, "is owing to the preternatural *contraction* and *relaxation* of the muscles about the hips." Physiologists would admit, I suppose, that a relaxation of muscles might allow a disordered limb to become longer, and that a contraction of muscles would shorten it. Then if to lengthen a limb the muscles must be relaxed, such should have been the conditions of these organs in our case; that is, the muscles about the hip hip must have been relaxed. But if these muscles had been relaxed, the limb would have been suspended from the body, all the joints being straightened, like those of a paralytic, which was not the case; for, instead of hanging down, it was actively and permanently drawn forwards towards the body. Whence it follows that the complaint could not have been simple relaxation.

Now take the case of contraction. If any one muscle were preternaturally contracted, it would undoubtedly *shorten* the limb, although the other muscles were relaxed; a contraction of the Biceps bends the arm, a contraction of the Tibialis draws up the foot, and a contraction of the Psoas magnus would shorten the lower extremity. Thus it follows that an elongated limb cannot coexist with a preternatural contraction of muscles; but the plaintiff's limb was elongated, and of consequence his affection could not have arisen from a preternatural contraction of the muscles.

3. An injury of which the precise nature is obscure.

"It is difficult to determine," says the deponent A., "in the case of injuries of the hip, precisely what the injury of the bones is; but it has frequently happened within my knowledge, that by a fall directly on the hip joint, though the bone was not dislocated, as was evident by the natural position of the foot and limb generally, and from its being moved by the hand of the surgeon in all directions, yet the patient has never recovered from his lameness, and in several instances they have never been able to walk afterwards." The object of these remarks is, I suppose, to intimate the suspicion that Mr. Lowell's case might be one of these obscure injuries of the hip, in which it is difficult to determine "precisely what the

injury of the bone is." Though to compare the known with the unknown is the province of mathematics, and cannot be expected of surgery, it will not, I think, be impossible to show, first, that the real nature of these cases may be discovered by careful investigation; and second, that the case of Mr. Lowell is evidently different from these.

To this kind of accident, I would then ask the attention of surgical practitioners. It usually occurs in aged persons, from a fall directly on the trochanter. The patient, after the occurrence of the accident, finds himself lamed, and the hip painful. On moving the limb, great pain is produced in the joint, and often a spasmodic affection of the muscles, which in some instances manifests itself also when the limb is not moved. The circumstances most to be noted, are, that the limb is neither shortened nor turned out, nor is there any grating of the bones; yet the patient does not at all recover from the lameness; on the contrary, it for some time increases. Ultimately, indeed, the foot turns out, and the limb shortens slowly. The patient never regains the natural use of the limb.

Is not this accident a fracture of the neck of the thigh bone, within the capsular ligament? But why does not the limb become shorter, and the foot turn out, and the fractured bones grate, as usual in such cases? Because, we answer, the capsular ligament is not torn. Therefore, the limb cannot at first become shorter, nor can the foot turn out materially, nor the broken pieces move on each other to cause a grating.*

The shortening takes place afterwards, from the contraction of muscles, and the gradual absorption or wasting of the ligament.

When the capsule is torn through at the time of the fracture, the ordinary phenomena of shortening, &c. present themselves at first.

I may venture to say then, that, whenever these obscure injuries are followed by the alleged effects, that is, by a permanent lameness, or an inabi-

* When the grating has been ascertained, and its situation distinguished, there can be no doubt of the existence of fracture of the neck of the thigh bone, even though the shortening of the limb, and other concomitant circumstances, should not present themselves; but I beg leave to say that I have known instances of fall on the trochanter, in which the grating could not be distinguished, yet a permanent lameness and shrinking of the limb occurred. Can there be a reasonable doubt as to the nature of these cases? if they are not fractures of the neck of the thigh bone, what are they? They cannot be caused by a rupture of the inter-articular ligament. A fracture of the socket is attended with symptoms incomparably more violent. No one will consider them as the phenomena of dislocation. They must, I think, be fairly considered as fractures of the neck of the thigh bone, though not accompanied with the most common symptoms of that accident. In a long and interesting conversation on surgical subjects, which I had last summer with a gentleman, whose genius and practical skill do honour to this country, as they would to any other, and of whose character I cannot sufficiently express my respect, I mean Dr. Physick, of Philadelphia; I stated to him the general appearances in such injuries of the hip, for the purpose of eliciting his opinion; not referring to the plaintiff's dislocation, which was not in my mind at the time. Dr. Physick promptly replied, (my much valued friend, Dr. Horner, the anatomist, being present,) that they were undoubtedly instances of fracture within the capsular ligament of the hip joint.

The controversy between the English and French surgeons, in regard to the possibility of a union in the fracture of the neck of the os femoris, is well known; and to those who have attentively read what Sir Astley Cooper has written, it will appear to be a point settled, that true fractures of the neck of the thigh bone, within the capsular ligament, do never unite. In Richerand's late work, there is a curious passage relating to this dispute, which shows that he is not willing to adopt the opinion of Sir Astley on this point, though he praises him on others. "Such persevering researches," says Richerand, "to attain the best means of treating fractures of the neck of the femur would have been puerile, if, as Sir A. Cooper has said in a recent work, these fractures were not susceptible of consolidation. How shall we explain so strange an assertion of this celebrated English surgeon, who reads little, it is true, but might have seen in all anatomical museums, whole *ossuaries* of fracture of the neck of the thigh bone consolidated perfectly, or partially."

lity ever to walk afterwards, they are truly cases of fracture of the neck of the thigh bone. Then, if we make any other comparison, it must be with the signs of this accident, and we may compare the symptoms of our case with fracture of the neck of the thigh bone with the usual symptoms, with fracture of the neck of the thigh bone with unusual symptoms, and with fracture of the neck of the thigh bone without distinct symptoms.

In the first case, namely, the fracture of the neck of the thigh bone with the usual symptoms, every surgeon knows that the limb is *shortened*, the foot is remarkably turned out, and the broken bones produce a grating. This is quite different from the plaintiff's case.

In the second, or fracture with unusual symptoms, the foot is *turned in*, the limb is shortened, though slightly, the crepitus, or grating, may be felt.* This case will not, I suppose, be thought to resemble that of Mr. Lowell's.

The third is, when there are no symptoms of a distinct character, and this seems to be the case more especially alluded to in the deposition. If there are no symptoms attendant on the accident, and none afterwards, the case is very different from ours, in which the symptoms are strongly marked. And again, if, though there were no symptoms immediately following the accident, yet some presented themselves at a subsequent period, they would always, in a case of permanent lameness, be accompanied, as already shown, with more or less shortening of the limb, which is the state opposite to that of the plaintiff.

We might suppose any other possible injury of this part, as, for example, injury of the orbicular or the internal ligament, inflammation of the synovial membrane of the joint, contusions of the muscles, or tendons; and show in a satisfactory manner, I apprehend, by comparing the symptoms, that none of them resemble the strongly marked characteristics of the plaintiff's injury.

Thus it seems, that a comparison of the features of this, with those of the various affections of the hip joint, presents no accordance of the former with those of any known or supposed injury of this part, except a displacement of the limb in a direction downwards, or below its socket; and I may add, that out of nine cases of dislocation of the thigh from violence, which have fallen under my notice, no one has exhibited more distinct marks than this, of a change of relation between the head of the bone and its natural cavity.

Some individuals may be disposed to believe, that the direct testimony of professional men would be more satisfactory than any argumentative discussion of the merits of a case not easy to comprehend, without bestowing on it more time and attention than they are willing to give. They ask for the opinion of competent and impartial judges, who have had an opportunity of actual examination of the disordered parts. Unnecessary as the more direct application of this test may seem, I cannot decline any mode of trying the question, which appears calculated to dissipate the obscurity that has gathered over it.

From the account we have already given, it appears that the medical gentlemen who examined, or were supposed to have examined, this case, delivered opinions absolutely opposed to each other. In order to form a just estimate of these clashing judgments, we must consider the number of individuals on each side, together with their capacity and opportunity for judging, and the degree of impartiality with which they might be supposed to view the facts.

* This peculiar turning in of the thigh, in certain instances, has been said to be owing to the fracture being on the outside of the insertions of the muscles rotating outwards; so that they lose their control of the limb, while some of the inward rotators are still attached to the bone, and must of course draw the limb inwards.

Four medical practitioners have deposed or testified, that the symptoms did *not* arise from dislocation, but from injury of the pelvis, or some other cause.

The first of these four is the deponent A. The testimony of this deponent should be considered very important, in what relates to experience in surgery, and knowledge of anatomy. On the other hand it must be noticed, that he does not speak of having seen more than one case of hip dislocation, so that his experience, on *this subject*, loses some of its weight. Then as to the period of examination, it will be recollected, that he did not see the patient till *nine months after the accident*, when considerable changes of appearance must necessarily have occurred. Further, that he examined once and once only.

The second deponent is the gentleman who was *present* at the examination of the deponent A. His deposition is evidently grounded on the opinion of the other, and must be considered as adding little if any weight to it.

The third and fourth practitioners on this side, never examined the patient. They founded their opinion on observations not stated, but which may be presumed to have been drawn from a cursory sight of the plaintiff in the court house, at a distance of time not very far from three years from that of his injury. They could not, therefore, be expected to be very good judges of his difficulty.

From this view it appears that only one of the four actually examined for himself, and it must follow that we cannot admit the testimony of more than one individual as affording any direct support to the negative opinion, that this bone was not displaced.

Let us next consider the positive testimony.

In presenting the evidence in favour of the dislocation, I am compelled to place my own before that of my colleagues. Not that I would wish to have it considered as of more weight than that of other gentlemen, but because my opinion was formed and given before theirs; and if it were an erroneous one, I should desire to take the imputation from them to myself. The comparison I wish to make will relate to two points: first, the epoch of the examination; second the extent or degree of this examination.

As to the first point, the epoch of examination, it has been already stated, that I examined the patient three months from the occurrence of his accident, soon after he had begun to move about from his confinement, while he was still on crutches, and before he had made any great use of his limb. The deponent did not examine till nine months from the accident, six months after my examination, when the patient had learned to use his limb considerably, and when, of course, all its appearances must have undergone material alterations.

Secondly, as to the extent of the examination. When the patient was first subjected to my inspection, I adopted various modes of satisfying myself what his injury was, but I would not decide definitively at once. I took time to examine the matter, looked into Sir Astley Cooper's book, and obtained all the information I could; then I examined the patient a second time, put him into various positions, examined the prominences of the bones, gave his limb all the motions it would receive, and then made up my judgment. After this I had a third and a more full opportunity of exploring the nature of the case; he was placed on a table, and operated on for a reduction of the limb, between one and two hours. During this time the limb was moved in various directions, the prominences and depressions of bones felt and indicated to gentlemen around, and by some among these, the head of the bone itself was felt, or supposed to be felt, and pointed out. The deponent A., on the other hand, made but a single examination, and that in the presence of only one medical person, so far as we are told; this examination was never repeated.

The testimony next to be considered on this side, is that of the consult-

ing physicians and surgeons of the Massachusetts General Hospital; namely, Drs. David Townsend, Thomas Welsh, James Mann, and William Spooner. The patient, after having been conveyed to the Hospital, was submitted to the inspection of these gentlemen. They were desired to examine whether the bone were dislocated or not, and to consider whether, if so, any attempt to reduce it were justifiable. They deliberately made their examinations, and pronounced that a dislocation did exist. As to the reduction, they were all of opinion it could not be effected, but thought if the patient insisted on it, that it would be justifiable to give him the trial. This trial was made before them, they had opportunity of again seeing and examining; they did examine repeatedly and constantly through the whole operation; and, though "the bone was not reduced," they did not the less believe it was out of place, and I presume they continue to believe it unto this day.

Perhaps it will be said that these gentlemen were influenced by the judgment I had already given, or that they were unwilling, through delicacy, to make any opposition to it. In reply to such remarks, I must say, that they have always been in the habit of very free discussion with me, on doubtful questions in the Hospital, and from these discussions I have often received much light, and the patients much advantage. My habits of intercourse with most of the gentlemen have not been sufficiently close to give me the means of operating on them through the medium of private friendship, since with some I have not exchanged a word for years excepting at these consultations. They are none of them of an age to be swayed by me, contrary to their own judgment, and have had experience enough to enable them to make up their minds on cases like this with perfect independence.*

* These physicians are in truth as venerable for age as they are for experience, and public service; and I cannot but regret, and I trust every man of character who reads this trial will also regret, that the attorney for the defendant should have indulged himself so freely in expressions derogatory to the reputation and injurious to the feelings of such gentlemen. In order to remove the unfavourable impressions which the course pursued by him may have made in regard to them, I shall take the liberty to state who they are.

Let it be remembered that the consulting physicians at the Hospital, in the part they took in this case, were performing a public and a gratuitous service; moreover, that the patient and his surgeon, the plaintiff and defendant, were utterly unknown to them.—Three of these gentlemen were surgeons in the army of the revolution. Dr. Townsend, the friend of the late governor of this state, was in the American army during the whole of the war of Independence, and for the last eighteen years has been surgeon of the Marine Hospital of this department, where he must have had great opportunities for surgical experience. Dr. Welsh was also surgeon in the American army, and has been for a great number of years health physician of the port of Boston, has held many honourable offices in the profession, and among others that of Vice President of the State Medical Society. Dr. Mann was also surgeon during the whole revolutionary war, and held a distinguished surgical rank in the last war with Great Britain, and does so at this time. He is very honourably known as the author of a valuable treatise on the Cholera of infants, of Medical Sketches of the late war, and other meritorious publications. Dr. Spooner is an Edinburgh graduate, of about 1783, a gentleman distinguished for his frank and independent character, and from whose sentiments I take pleasure in saying I have derived particular benefit in the Hospital.

Are the names of such persons proper subjects for the jeers of a lawyer, to be thrown out in a court of justice, and afterwards distributed through the country? and would the strength of the argument have been weakened by the omission of witticisms on the characters of these individuals?

Whatever may have been the prejudices existing in the minds of the court and jury, I think I may venture to say, that if at the time of this affair it had been necessary to select four gentlemen qualified by their general reputation and their experience to judge in this case, no other four practitioners could have been more properly chosen from the state of Massachusetts;—I may go farther, and say, from the whole of this section of the country.

Respectable as they are, and laborious as have been their services, they are not, as the attorney tells us, when he would excite sympathy for his client, "nursed in the lap of ease." Every one of these gentlemen, after a life of severe public and private duties, is still called on to exert himself in an honourable slavery to the labours of his office and profession.

Let me repeat that the four consulting physicians above named, saw this case three months after the injury; they had most of them seen dislocations of this kind, and on a full examination of the disordered part, were unanimously of opinion that the bone was out of joint. Compare their judgments, thus formed, with that of the deponent B., who was *present* at an examination; and with that of the witnesses C., and D., who never examined, nor were even *present* at any examination.

The affirmative testimony is not finished. The history of the case already given, shows that the plaintiff afterwards applied to a very experienced practitioner of surgery, who was so satisfied of the existence of a dislocation, as to proceed, in company with other practitioners and students, to attempt the reduction of the limb by the most thorough and strenuous efforts. This was a short time after the trial at the hospital.

In fourteen days more, the persevering plaintiff was induced to apply to one celebrated for his practical skill in reducing bones. He also made an attempt to reduce the dislocation, and of course we have the strongest reason to presume he believed in its existence.

The whole of this mass of opinion in favour of the existence of a dislocation, was formed on examinations which took place about three months only from the time of the accident.* When this fact is duly considered, and when in connexion with it we regard the number of those who examined, the nature of the examination they made, their respective ability to form a solid judgment, it must I think be admitted, that it would be difficult to have a greater weight of testimony as to any fact of this description; and that on the other side, the negative evidence of the deponent A., with the addition of that of the deponent B., founded on an examination six months later, must be viewed rather as a contrast to the affirmative, than a refutation of it.

We have something still to add. The plaintiff, after the third attempt made on him, quitted Boston to return to his own state. On his way home, it appears that he took the advice of Dr. Benjamin Brown, of Waldoboro, in the state of Maine, a practitioner of great experience and respectability. His opinion as expressed in his deposition was, that the "plaintiff's inability or lameness is a simple luxation of the head of the thigh bone, and that with skilful treatment and prudent management at the time of recent injury." "the plaintiff might now have the use of his limbs."†

Finally, we have the testimony of Dr. Estabrook, to deprive us of a supposition which otherwise might have been entertained. Sir Astley Cooper relates, as we have seen, the case of a man, who having a dislocated hip, was subjected to the skill of many surgeons without success, and at last, after a lapse of years, was suddenly cured by the bone slipping spontane-

* It might be thought I should adduce the practice of the defendants in support of the opinion that the limb was actually dislocated at first, contrary to what appears to have been the notion of the deponent A. On the other hand, it might with equal truth be said, that their subsequent practice is opposed to the doctrine of a dislocation existing at present. What degree of weight is to be attached to either, I do not think it necessary to inquire at present. It is sufficient perhaps to show I have not forgotten that these gentlemen did make an examination of the case; though not after it became a subject of litigation.

† On this very distinct and positive testimony, the defendant's attorney has commented, with many sarcastic expressions.—The testimony is, however, really of much weight. Dr. Brown was called to see the patient by himself; he had experience on the particular subject in question, since it seems he was "in the medical and surgical department of the United States during the term of five years, during which," says he, "I was in the land and sea service in the revolutionary war; I have been in several engagements both on the sea and on the land, in which I have performed many surgical operations. I have seen and reduced several dislocations of the head of the os femoris or thigh bone." So far as I can learn, there is not in the state of Maine a medical gentleman more distinguished for professional experience and integrity of character than Dr. Brown.

ously into its socket. Some ingenious persons might think it proper to intimate, that during the six months between our examination and that of the deponent A, the plaintiff's bone might have returned into its socket in a similar manner. But Dr. Estabrook deposes, that "Jan. 23, 1822 [1823,] he examined Lowell's hip and *found it dislocated; the head of the bone was out of the socket;* with skilful treatment he might have recovered the use of the hip." The examination and opinion of this gentleman was as independent of ours in its circumstances as it was distant in time. None of us, to my knowledge, communicated with either of the physicians of Maine, whose depositions are here given.

This testimony is too forcible to require any rhetoric to display it. There is however one more fact to add to its weight. The patient still lives; still carries about with him the signs of his injury, obvious to every beholder; and although the gradual formation of an artificial socket will diminish his trouble, and obscure his symptoms, he must have an elongated and distorted limb while he lives.

Having thus concluded my remarks concerning the first of the two propositions laid down above, viz. *that no dislocation existed in the case of Mr. Lowell,* and having as I trust made it sufficiently evident that a dislocation did exist, I would now state that this is really the essential point to be proved. As to the particular species of dislocation, it is much less important that the profession or the public should agree with me in opinion. In the trial, however, it happened, that the defendant's attorney, availing himself of some extracts from the work of Sir Astley Cooper on dislocation, made it appear, that in the opinion of this author, no such species of dislocation could exist, as that I had assigned. Whence the court and jury were made to believe, that, if we had been in error as to the kind of dislocation, we might have been so even as to the existence of any dislocation. Had those who examined the case, been present as witnesses in a court of justice, it would not have been difficult for them to have made it appear, that the two questions might and ought to have been separated from each other. As the matter actually stands, I am called on to show more precisely the grounds on which I formed my opinion of the particular kind of displacement; and this appears to be the more necessary, because the attorney has employed, in regard to this opinion, terms applicable to the most gross ignorance of surgery and anatomy. "The account given by Dr. Warren," he says, "will prove to be a complete fallacy," "a most egregious error." "Its refutation will be established on the most obvious principles of anatomy, which, it will appear, have not only been disregarded by these learned doctors, but that their conclusion has been adopted in absolute defiance of them." Let us again enquire whether this language is authorized by facts.

In order rightly to comprehend the case, it is necessary to attend to the distinctions between the different kinds of dislocations of the thigh bone, or os femoris, on the hip bone, or os innominatum; and to understand the nature of these distinctions we must have some acquaintance with the form of the bone principally concerned in them.

This is called os innominatum. It is a large flat bone spreading out above, and contracted below. Towards the lower part is seen that deep excavation, or socket, called acetabulum, which receives the head of the thigh bone. To render its anatomy more intelligible to those not versed in anatomical studies, I have annexed a representation of it, with indications of the parts most connected with the questions to be discussed.

PLATE I.

A lateral view of the os innominatum of the left side, reduced about one

third from the natural size of this bone in an adult; the acetabulum being placed on a level with the eye.

A. The acetabulum.

B. The posterior, inferior, spinous process of the ilium.

C. The tuberosity of the ischium.

D. The foramen ovale.

E. Anterior extremity of the pubes.

F. Spinous process of the ischium.

aa. A line denoting the situation of the ligament, which extends from the tuberosity of the ischium, to the posterior, inferior, spinous process of the ilium, forming the posterior boundary of the ischiatic notch.

bb. A line denoting the situation of the ligament which passes from the spinous process of the ischium, to the side of the os sacrum, dividing the ischiatic notch into two unequal parts.

ccc. Three lines marking those parts of the os innominatum where the ilium, ischium, and pubes are connected by cartilage, before their ossification is effected.

dd. A circle marking that part of the bone, on which the head of the femur rests when it is dislocated backwards and upwards on the Dorsum ilii.

ee. A circle marking the situation of the head of the femur, when dislocated backwards to the superior part of the ischiatic notch.

ff. A circle marking the situation of the head of the femur, in a dislocation backwards and *downwards* on the inferior part of the ischiatic notch, and os ischium.

gg. A circle showing the situation of the head of the femur, when dislocated forwards and upwards, on the os pubis.

hh. A circle showing the situation of the head of the bone, in a dislocation forwards and downwards, on the foramen ovale.

In this view the bone appears as one, and is actually so in the full grown body, but in the earlier periods of life it consists of three separate bones, whose connection is formed by cartilage. The three bones are called ilium, ischium, and pubes; they meet together in the socket. The ilium is the uppermost and broadest; rounded at the top, and gradually contracting below. The ischium is the lowest of the three, and has the letters *C*, and *F*, in the plate. The pubes is the anterior, or front bone, and has at its extreme part, the letter *E*. In order more distinctly to indicate the extent and bounds of each of these bones, three straight lines have been drawn from without to the interior of the socket, each marked *c*. The part above the two superior lines, is ilium; that below the right hand line, ischium; and that below the left hand line, pubes. The three bones, uniting, form the socket. When the human body approaches the adult age these bones coalesce, or co-ossify into one, called the os innominatum. The broad upper part of it is denominated the dorsum of the ilium, the lowest part is called the tuberosity of the ischium. The projection, on the right, marked *B*, is the posterior, inferior, spinous process of the ilium. The projection marked *F*, is the spinous process of the ischium. *D*, shows imperfectly the situation of the foramen ovale. The line marked *a a*,* is the posterior boundary of the ischiatic notch, and shows the situation of the larger sacro-sciatic ligament. The line *bb* shows the situation of the smaller sacro-sciatic ligament. The last divides the ischiatic notch into two parts, an upper larger, and a lower smaller, both of which are converted into holes, or foramina by the first.

In order to prevent any misapprehension as to the last mentioned point of anatomy, I shall illustrate and confirm what has been said in regard to it,

* Lines can give no idea of the form, and an imperfect one only of the situation of these ligaments, for the breadth of the larger ligament varies from half an inch to more than two inches. The lines serve, however, to show the manner in which they constitute the two foramina.

by the authority of the best authors and anatomists. This is especially necessary, for the two following reasons; 1st, that the part alluded to is more particularly concerned with the subject we are to examine, and 2d, that there is evidently some misunderstanding actually existing in regard to it, which may be one cause of the apparent difference of opinion, between different surgical practitioners and authors. For the purpose of illustration, I shall refer to the most celebrated standard continental works on anatomy, since it is well known, by English physicians, and regretted, that there is not an accurate and minute system of anatomy in the English language.* Our American anatomists have therefore employed, as far as I know, the publications of the continental writers of Europe; and this has been my practice, since I had the honour to be a humble teacher of the science.

Than Soemmerring of Goettingen there is no greater living authority in anatomy. His elegant monographic treatises on the eye, on the ear, on the organs of taste and of smell, as well as on other subjects, are known to all anatomists. His general treatise on anatomy, entitled "De Corporis Humani Fabricâ," contains the following description of the part alluded to, as particularly implicated in this discussion.†

"*Os ischium, or the ischiatic part of the os innominatum.*

"It is composed of two branches, an anterior smaller, and a posterior larger. The anterior branch grows broader and thicker gradually, from the place where the descending branch of the os pubis passes into it, and ascending is connected with the posterior branch. Its anterior surface is rough, the posterior smooth; the superior edge sharp, the inferior broad.

"The external surface of the posterior branch presents an obtuse crest, transformed into the prominent margin of the acetabulum, and, towards the posterior parts, the tuberosity of the ischium, encrusted with cartilage, beginning by a contracted part, and, gradually spread out, terminated by an oval circuit, and having the edge towards the external and posterior parts turned over. Between the acetabulum and the tuberosity of the ischium, there is a furrow, which receives the tendon of the obturator externus muscle. Afterwards, in order to form the posterior part of the acetabulum, it

* The Edinburgh system of anatomy is a compilation of a heterogeneous kind, very deficient in the descriptive part of the organs most concerned in surgical practice. This is the most popular work in Great Britain. But I cannot help expressing surprise that among the numerous translations of French and other continental books, a translation of the system of Soemmerring, or of that of Sabatier, Boyer, or Bichât, is not to be found.

In our own country the late Dr. Wistar has published an excellent treatise on anatomy, but the descriptions were neither intended nor required to be minute, like those of the authors mentioned above.

† "*Os ischii, sive pars ischiadica ossis coxæ.*

"E duobus ramis, priori minori, posteriori majori componitur. Prior ramus, ab eo inde loco, quo descendens ossis pubis ramus in eum abit, sensim, latior, crassior evadit, ramoque posteriori adscendendo adjungitur. Prior ejus superficies aspera est, posterior lævis; margo superior acutus, inferior latus.

Rami posterioris externa superficies cristam obtusam, in prominentem acetabuli marginem mutatam, et posteriora versus tuber ischii cartilagine crustatum, initio contracto ortum, sensimque diductum, ovali ambitu terminatum, et ad externa, et posteriora, circumvolutum exhibet. Acetabulo et tuberi ossis ischii sulcus, tendinem musculi obturatoris externi excipiens, interponitur. Post ad constituendam acetabuli posteriorem partem fornicatur, in duas partes abit in *inferiorem minorem*, sive incisuram ischiadicam inferiorem, quæ musculum obturatorem internum excipit, et in superiorem majorem, ellipticam, sive incisuram ischiadicam superiorem, sive potius *iliacam*, minimam partem ab osse ischii, maximam vero, ab osse ilii constitutam; in quâ nervus ischiadicus, arteria et vena ischiadica et glutea ponuntur. Spina prominens, ischiadica vocata, incisuris interponitur.

is arched and passes off into two parts, an *inferior* smaller, or the *inferior ischiatic notch*, which receives the obturator internus muscle; and a *superior larger*, of an elliptical form, or the *superior ischiatic notch*, or rather *iliac* notch, (for the smaller part of it is constituted by the os ischium, but the greater part by the os ilium,) in which the sciatic nerve, the ischiatic and gluteal artery and vein are placed. The prominent spine of the ischium, as it is called, is placed between the two notches."

The anatomical work of Boyer appears to me a model of distinct and accurate description, and in these qualities not to be surpassed. In the account of the os innominatum we find the following passage.*

"Below this eminence" (that is, below the posterior, inferior spinous process of the ilium), "we see a considerable notch which makes *a part* of the ischiatic notch." In the description of the os ilium, he says, "the posterior edge presents also two notches, one superior, very small, (between the spinous processes,) and the other inferior, very large, which makes *part* of the ischiatic notch." Again, in speaking of the sacro-sciatic ligaments,† "the *two sacro-sciatic ligaments convert the ischiatic notch into two holes*, a superior larger, and an inferior smaller. The first is filled, in the recent state, by the pyramidalis muscle, by the gluteal vessels and nerve, and by the sciatic and pudic vessels and nerve. The second is filled by the obturator internus muscle, and by the pudic vessels and nerve."

The "Dictionnaire de Médecine," now publishing in Paris, is edited by twenty-six distinguished professional gentlemen, among whom are Beclard, Chomel, Orfila, Pelletier, &c. This work must of course speak the language and opinions now received by the profession in France. In their description of the os innominatum they observe, that "behind the spine of the ischium is a deep notch, and another smaller one, forming a smooth groove, separates this spine from the tuberosity; these notches are known by the names of the *great and little ischiatic notches*."‡

To these authorities I shall add that of Meckel, a name long celebrated among the anatomists of Europe. In a work lately published by the professor of Halle, which has just appeared in Paris, translated into the French language, and is considered a valuable acquisition to the medical profession,§ we find the nomenclature proposed by Soemmerring, adopted and established; for, in the description of the ilium he thus speaks of the superior notch:

"The inferior edge (of the ilium) is deeply indented: it forms the posterior and superior part of the *iliac notch*.‖

In describing the ischium; "Between its tuberosity and the inferior edge of the cotyloid cavity, is found a deep impression on the outside. We per-

* "Plus bas est une autre éminence, formée par la partie postérieure de la facette qui s'articule avec le sacrum. Cette éminence est médiocrement saillante, arrondie, tranchante, et porte le nom de l'épine postérieure et inférieure de l'os des îles. Au dessous de cette éminence on voit une échancrure considérable, qui fait *partie* de l'échancrure sacro-sciatique."

† "Les deux ligamens sacro-sciatiques convertissent l'échancrure sacro-sciatique en deux trous; un supérieur plus grand et un inférieur plus petit. Le premier est rempli, dans l'état frais, par le muscle pyramidal, par les vaisseaux et le nerf fessier, par le vaisseaux et nerfs sciatiques, et par les vaisseaux et les nerfs honteux. Le second est rempli par le muscle obturateur interne, et par les vaisseaux et le nerf honteux."

‡ Une échancrure profonde existe en arrière de l'épine sciatique, et une autre plus petite, formant une coulisse de glissement, sépare cette épine de la tubérosité: ces échancrures sont appelées la *grande* et la *petite échancrues ischiatiques*.

§ I am happy to learn that this excellent work is proposed to be given to the American public, in an English translation, by Dr. Bradford of Cambridge.

‖ Le bord inférieur est fortement échaneré: il forme la partie postérieure et supérieure de l'*échancrure iliaque* (incisura iliaca, s. *iliaca superior.*)

ceive also between it and the spinous process, at the internal face and the posterior edge, a notch called *inferior iliac*, or *ischiatic*."*

The quotations made above must be sufficient to satisfy any anatomist that these authors applied the term ischiatic notch, not, as some have thought, to the space above the letter *F* in Plate I, exclusively, nor to the space below, exclusively, but to both these parts. Boyer considers the whole to be one notch, that is, from *a* to *a*, divided into two parts by the sacro-sciatic ligament; Soemmerring and the editors of the "Dictionnaire" make two notches, which *he* calls superior and inferior, and *they* great and little, ischiatic notches; and Meckel gives the name of iliac notch to the whole indentation, that of superior iliac, to the upper notch, and of inferior iliac or ischiatic, to the lower notch. The nomenclature of Meckel is perhaps the most exact, for the reasons assigned by Soemmerring, namely, that the superior notch is formed principally by the ilium, and not principally by the ischium. If the term ischiatic is retained, it must be applied to the notch as a whole, or it cannot apply at all to the upper part, this being placed in a different bone. The impropriety of confining the term ischiatic notch to the upper portion is too obvious to require any remark. If its application were restricted beyond what has been done by these writers, it should be to the lowest part of the notch, which only is really contained in the bone ischium.†

Dislocations, or displacements of the thigh bone from its socket, may take place in any part of its circumference. When we consider the form of this socket, of which some notion may be had from Plate I; and also the form of the head of the thigh bone, as it may be seen in Plate II, it will readily be understood, that so far as regards the bones, there is no natural obstruction to a displacement at any part of the circular rim of the acetabulum. This opinion is remarkably confirmed by the fact, that the point where the greatest anatomical obstruction exists to the escape of the bone, is precisely the place where dislocations most frequently occur; that is, at the upper and outer part of the socket. Here the ridge of the acetabulum is more elevated than at any other part; yet it is here that the dislocation on the dorsum of the ilium happens, the dislocation upwards and backwards, a kind of displacement which occurs, according to Sir Astley Cooper, in three out of five instances of luxation of the hip joint. But although the thigh bone may be thrown out in any direction, it cannot rest with equal facility on every point of the os innominatum. The action of the muscles, or movements from external causes, would readily carry it back to its place, were it not hitched by bony inequalities about the circumference of the socket, which serve to engage the displaced head of the bone, and to retain it in four different situations, according to most authors and surgeons. In these places it is confined by the inequalities of the bone, aided by the strain put on muscles extended beyond their ordinary condition, and on the lacerated portion of its ligaments.

The four situations, taking them in the order of frequency, are,

* Entre cette tubérosité et le bord inférieur de la cavité cotyloide, se trouve une gouttière profonde en dehors. On aperçoit aussi entre elle et l'épine, à la face interne et au bord posterieur, une échancrure appelée *iliaque inférieure* ou *ischiatique* (*incisura iliaca inferior*, s. *ischiatica.*)

† The learned attorney for the defendant alludes, with signs of satisfaction, to expressions made use of by me, in which the idea is conveyed of a dislocation backwards and downwards in the *ischiatic notch*, as if this notch did not extend below the level of the socket. It is to these expressions I suppose he intended to apply the epithets, "absurd, unaccountable, incredible," and it is this idea he would call "a perfect fallacy," "a most egregious error," "capable of being refuted on the most obvious principles of anatomy." An accurate notion of the anatomy of the part must convince him that these epithets were misapplied.

1. On the Dorsum of the ilium, as indicated by the circle *dd* in Plate I. This is called upwards and backwards.

2. In the foramen ovale, or forwards and downwards, indicated by circle *D hh.*

3. On the pubes, or upwards and forwards, circle *E gg.*

4. On the ischiatic notch, downwards and backwards, circle *F ff.* Besides these four cardinal dislocations, others have been described. The dislocation directly backwards, or backwards and a little upwards, on the superior part of the ischiatic notch, or what should be called the *iliac* notch. The last is described in Plate I. by that part of the outline included in the circle *ee.* No doubt the head of the thigh bone may be sometimes lodged in places different from any of those just mentioned. Its displacement downward has been described by various authors, ancient and modern. I have myself seen it in a situation in which, although I think the sort of occurrence has been noticed by some author, I could with difficulty have conceived it to remain, had I not witnessed it; that is, upwards and outwards, just on the edge of the socket. From such a situation it might have been supposed, that the slightest motion would have thrown it into its place. Yet there it remained, and still, I presume, remains, for, when I saw it, too long a period had elapsed after the injury, to think of an attempt at reduction.

The four dislocations above mentioned, are represented in the annexed Plates.

Plate II. A lateral view of the os innominatum, with the upper extremity of the femur, in the situation in which it is, when dislocated backwards, or nearly so, the head of the bone being in the upper part of the ischiatic notch, or properly speaking, in the *iliac* notch.

Plate III. The same view of the os innominatum as in Plate II: *a*, a dotted outline of the upper extremity of the femur, showing its position in a dislocation backwards and upwards upon the Dorsum ilii; *b*, a representation of the upper extremity of the femur, in its position, as dislocated backwards and *downwards* to the lower part of the ischiatic notch.

Plate IV. A front view of the left side of the pelvis; representing also the position of the femur, when the head of the bone is dislocated forwards and upwards upon the os pubis.

Plate V. A view of the pelvis, as in Plate IV, and of the femur as in a dislocation forwards and downwards, with the head of the bone lodged in the foramen ovale.

While some authors have increased the species of this dislocation beyond the number commonly admitted; others, and these of no small note, appear disposed to diminish them, or are unwilling to admit the existence of all these species.

Mr. William Hey of Leeds, excelled by few men of his time as a practical surgeon and writer, and what is more to his credit, by few in his general character, seems to admit only two kinds of dislocation: "A dislocation of the os femoris at the hip," says he, "may happen two ways, forwards and downwards, or backwards and upwards." "Seven instances of the latter, and three of the former are all that have occurred in my practice." "Thirty years had nearly elapsed, after the opening of the General Infirmary of Leeds, before any patient was brought to it with a dislocation forwards and downwards. Nor had I, during a period of thirty-eight years, seen that accident in private practice."

Mr. Charles Bell, in his system of operative surgery, describes two kinds of dislocation only, namely, upwards and downwards.

Mr. Benjamin Bell, author of the System of Surgery, thus expresses himself on this subject: "It is said by authors, that the head of the femur may be luxated in various directions, namely, upwards and backwards, upwards and forwards, downwards and backwards, downwards and forwards, and I may add, directly downwards; but I believe few practitioners have met

with an instance of the first and third." He further says, that all practitioners admit the bone to be most frequently dislocated into the foramen ovale. "In the dislocation downwards, the leg will be longer than the other, and the trochanter lower, but the knee and toes will retain nearly their natural situation." It is remarkable that of the two species of dislocation, which Mr. Bell seems to doubt, one has been proved by the observation of other surgeons to be more common than all the rest taken together.

The difference of opinions among surgeons, eminent for their knowledge and experience, requires some explanation. This will be found, I apprehend, in the fact, that surgical practitioners of great experience are naturally inclined to the belief, that they must themselves have witnessed every important variety of accident and disease, and that what *they* have not seen does not exist. What other explanation can we put on the assertion of Mr. Bell, that the dislocation on the Dorsum ilii has rarely been witnessed, and that the dislocation in the foramen ovale is the most common species? Or how else can we understand that Mr. Sharp, the greatest English surgeon of his time, and highly esteemed at the present day, should have doubted the existence of any hip dislocation? Mr. Benjamin Bell did the same in the earlier part of his practice, till experience led him to a different conclusion. Such errors will teach us to view, with a reasonable degree of caution, the round and positive assertions of practitioners, who would put down established opinions by their own solitary experience; and they also show us the necessity of exercising our own judgments in a fair comparison and estimate of opposing facts, as observed by different authors. "Desault," says M. Richerand in his late work, "was too easily persuaded, that the surgeon of the greatest hospital in the capital must necessarily be the greatest surgeon in the world; supported in this idea by his flatterers, he exaggerated to himself the importance of his labours, and renouncing the attempt to enlighten himself by the knowledge of his brethren, he founded his reputation on titles, of which every day diminished the value." Such must be the fate of every surgeon, who, from too great confidence in himself, undervalues the opinion of his professional brethren, and considers the whole circle of surgical knowledge to be comprised within the sphere of his own observation. How absurd would it be thought for a surgeon in Boston, practising in the midst of a population of 80,000 persons, to pretend to have seen every variety of accident or disease, which has occurred to the practitioners in the state, comprising half a million. He might have seen more than any three surgeons, or any ten, but that he should have observed all the variety of cases which has come within the cognizance of a thousand practitioners, would be thought a pretension too comprehensive to be allowed. So it would be equally inadmissible for a great surgeon in Paris, to pretend to have seen all which could happen throughout the world.

We shall now confine our inquiry more particularly to the kind of dislocation supposed to have existed in the case under consideration;—that of Mr. Charles Lowell of Lubec. This is already stated to have been taken by me to be a dislocation downwards and backwards, in the ischiatic notch, such as is indicated in Plate III, and the situation of which has been shown in Plate I, by the circle *ff*. The existence of this species of dislocation has been doubted, as we have just seen, by Mr. Benjamin Bell; but his opinion cannot have much weight in the case, since he equally doubts the existence of the dislocation upwards and backwards, well known to be the most frequent kind of this accident.

Besides him, we find three other surgeons of distinguished reputation, who have either questioned or directly denied the dislocation backwards and downwards. More than a century ago, John Lewis Petit, a distinguished surgeon in Paris, declared that he had never seen it, and that he

even believed it to be impossible. He admits, however, that it may exist when the muscles are paralyzed, and gives instructions how to effect its reduction. This doctrine of Petit does not appear to have been generally received; for the next authority to be found in its support, after the lapse of a century, is that of Boyer, an excellent surgeon, now living in Paris. In his treatise on surgical diseases, originally printed in French, translated and published in this country by Dr. Stevens of New York, he says: "We do not think the femur can ever be luxated downwards and backwards, but it may be carried in that direction after a luxation upwards and outwards.—That is to say, the head of the femur, thrown in the first instance on the external iliac fossa, may, if the thigh be afterwards adducted by any cause, slip down before the upward part of the ischiatic notch, but it can never reach the junction of the ilium and ischium." Whence it appears not only that Boyer denies the primary dislocation backwards and downwards, but also the primary dislocation backwards; while the latter is admitted by high authorities, who question the former. In truth, of the few surgeons who deny the dislocation backwards and downwards, there is no other that doubts the dislocation backwards. The whole surgical world seem to be against Boyer on this subject; and his authority in the case is consequently much diminished, especially as he falls into the error we have just now had occasion to notice, that of trusting too confidently to his own experience; for Boyer gives us no reason why the dislocation backwards, or the dislocation downwards and backwards, should either of them be impossible; he does not point out any physical obstacle to such an occurrence; he does not inform us that the socket is more elevated at the lower part than at the upper, or that any other condition of the bones is such as to present an insurmountable obstruction in this direction. We know, on the contrary, that such obstructions do not exist, and that the resistance to the exit of the bone, in an inferior, posterior direction, is less than in that which is the most frequent, the superior and posterior.

In the trial of this cause, the opinion advanced by me is opposed by the attorney for the defendant and his coadjutors; it is worthy of remark, however, that in his support he adduces neither of the authorities mentioned above, but only that of Sir Astley Cooper in his treatise on dislocations.*

* An attempt is made to bring forth the opinion of this distinguished surgeon, as uttered in the third edition of his works; to place it in the light of a discovery so recent as not to have found its way to the physicians of Boston, though in some wonderful manner revealed to the attorney and his coadjutors; and to represent this *revelation*, for so I think he calls it, as a refutation of the "absurd," "unaccountable," "incredible," error into which I had so unhappily fallen. What are we to think of such a proceeding? Was the gentleman who professes to have so perfect a knowledge of the subject, as to persuade himself he could put down and trample on the opinion of physicians, was he ignorant of such common works as Benjamin Bell, known to every student, and Boyer on the Bones, which has passed through two or three American editions? Or was this pretended discovery a manœuvre, thought to be well calculated to dazzle and blind the court and jury? The most charitable course will be to give the learned counsellor full credit for ignorance in this case; for I am not willing to believe an imposition so gross could be attempted.

The discovery imputed by these gentlemen to Sir Astley, is by him not claimed nor considered as a discovery; and the opposition in which it is placed to what I had advanced, exists more in the glowing language of the attorney than it does in fact. Whether there be any difference between the opinion found in Sir Astley Cooper, and that which had been given in my deposition, and, if any, how great the difference is, will appear on a more exact examination of the statement of this author. As the name of Sir Astley Cooper has been so often repeated in these remarks, and as these gentlemen have attempted to avail themselves of his authority to overthrow my opinion, it may be proper in this place to state how far we are willing to consider him as a general guide in matters of surgical science.

Sir Astley Cooper was a disciple of Mr. John Hunter, and an immediate pupil of Mr. Henry Cline, of St. Thomas's Hospital, the latter a gentleman, I believe still living, who

If it were necessary for the safety of my reputation to oppose doctrines emanating from such high authority, my task would be hopeless. Happily this is not the case; the opposition of opinion is more in appearance than reality. The attorney and his friends have arrayed Sir Astley Cooper in a hostile garb of their own contrivance. Strip off this foreign dress, and it will be seen there is no material difference between the opinions expressed in his work, and those which appear in my deposition. Sir Astley remarks that he had never in his long experience seen the dislocation backwards and downwards, and thinks it must be an unfrequent accident; but expressly admits its *possibility*. In this I should perfectly agree with him. He asserts that a dislocation in the ischiatic notch, or that which, according to the continental anatomists, is the *upper part* of the ischiatic notch, cannot be below the socket of the hip. In this, also, I must entirely coincide with him. In one place indeed he says, "It is to be remembered there is no such accident as a dislocation downwards and backwards," but this must be taken not in an insulated sense. To get the true meaning of the writer we must have in view all he says in relation to the topic. Now we find in the first part of his treatise (I speak of the third edition, that quoted by the learned attorney in his speech) the following passage: "A dislocation backwards and downwards has been described by some surgeons who have had opportunities for observation; but I have to remark that no dislocation of that description has occurred in St. Thomas's, or Guy's Hospital for the last thirty years, or in my private practice; and I doubt its existence, though I *would not deny the possibility* of its occurrence, but am disposed to believe some mistake has arisen on the subject." In a subsequent page the same remark is repeated, with the following additions and explanations. "If such a case does ever occur, it must be extremely rare. I cannot help thinking that some anatomical error must have given rise to this opinion, as in the dislocation downwards and backwards the head of the bone is described as being received still into the ischiatic notch; but this notch is in the natural position of the pelvis above the level of the line drawn through the middle

was distinguished, as a lecturer on anatomy, for his admirable perspicuity, and as a practical surgeon, for his calmness and steadiness in operation. By his own talents and industry Sir Astley elevated himself to the place of colleague with Mr. Cline, as a lecturer on anatomy, and he opened a separate course of surgical lectures given by himself only. On the resignation of Mr. William Cooper, surgeon of Guy's Hospital, a gentleman whose name I cannot pass over without expressing my respect for his excellence, Sir Astley was chosen by the governors of Guy's Hospital a surgeon of this noble charity; continued to be so more than twenty years, and has but lately relinquished this post. When he was elected to Guy's Hospital, I was engaged in the service of the institution under Mr. William Cooper, and had the honour of being the first to receive the commands of Sir Astley. From that time to the present I have been more or less in the habit of communicating with him, and have experienced an uninterrupted flow of kindness for myself, and of hospitality towards my countrymen, whom I have introduced to his notice. Nor have I been wanting in the respect due to so distinguished an instructor and friend; for probably I was the first to make his name known, and to spread abroad his reputation on this side of the Atlantic. And to those who have attended my lectures, I can appeal in support of the assertion, that I have quoted no other name more frequently and none with more respect. In truth, Sir Astley Cooper is entitled to all the respect that has been paid to him. He is not a mere surgeon, but a philosopher of the school of Bacon, a careful observer of nature, and a reasoner by induction from facts. His works are models of practical doctrine, without the least superfluous theory, or unnecessary discussion, and I have often thought and have expressed to him, that surgeons are much indebted to him, for giving to the public the fruits of his experience in the more common and frequent cases of surgery as well as in those which are rare. He is a decided friend to the American nation. If to these points of character we add that he is eloquent as a lecturer, and admirable as an operator; that his personal appearance is noble and imposing, few men would seem likely to inspire, and to have greater claims to general respect than Sir Astley Cooper.

of the acetabulum; and thence it is that the leg is shorter, not longer, where the bone is dislocated into the ischiatic notch."

From these quotations it appears, that Sir Astley Cooper, not having witnessed the dislocation backwards and downwards in his long course of experience, was naturally led to doubt its existence; that he expressly disclaims a denial of its possibility, and admits that it had been described by persons of observation, though he conceives its occurrence to be rare; and that he is disposed to think the supposition of such an accident may have arisen from a mistake as to the anatomy of the part.

Now it is obvious that the different representations made of this case by authors, have arisen from a different application of terms. Nothing can be more certain, than that, in ascribing the error to an anatomical mistake, and in showing this mistake to have consisted in the notion that the ischiatic notch was lower than the hip socket, Sir Astley Cooper alluded to what we all the *upper part* of the ischiatic notch; what Boyer and the French anatomists generally have described to be the upper part, what Soemmerring names the superior ischiatic notch, or, if rightly denominated, the *iliac* notch. That which Sir Astley calls the ischiatic notch, is really above the socket; but what the continental anatomists so name, extends both above and below. A dislocation in the upper part of it is above the socket, and the limb is shortened; a dislocation in the lower part, or on the surrounding bone, is below the socket, and in such the limb would be lengthened.

That such is the true explanation of Sir Astley's opinion of the subject, is, I have already said, obvious; for no one will venture to attribute to Sir Astley Cooper, or to any other anatomist, the assertion that the lower part of this notch, where the obturator internus muscle is situated, is above the level of the hip socket. It is in fact so much below it, that a dislocation in which the head of the bone was placed there, or thereabouts, would present a limb lengthened from two to three inches.

The explanation of the anatomical error being rightly understood, and also the declaration of Sir Astley Cooper, that he would not deny the possibility of this accident, it must follow, that the weight of his authority against its existence is greatly diminished, if not altogether removed; for we have no remaining objection on his part, but the fact, that he had not witnessed the very case; but Sir A. Cooper is too much of a philosopher to have made this the principal ground for denying its occurrence, as Petit very unceremoniously does. The latter tells us roundly, he has never seen such an event, and therefore it is impossible. But on what ground, let me ask, could Petit, or any one else, assert such an occurrence to be impossible? Where are the barriers to it, and what are they? Do they exist in the bones? I have already shown the reverse. Are they formed by ligaments? these are weaker below than above. Or in the muscles? they are not stronger in this than in any other direction. Where then is the insurmountable obstacle that renders this displacement downwards and backwards an impossible occurrence?*

* No doubt it will excite surprise in the minds of some persons, that in the vast extent of their practice, neither Sir A. Cooper nor Boyer has met with this dislocation, and we may be asked why they have not? the reason is plain; it lies in the rareness of its occurrence. We have no right to demand that one practitioner should have seen every accident and disease; life is too short for an experience so extensive; for while we are in the full career of observation, and are gratifying ourselves with the expectation that we are soon to see all there is to be seen, and know all there is to be known, death comes upon us and cuts all our experience short, and we are able to leave but a small part of it behind us. A multitude of instances might be adduced in illustration of this statement; I shall quote only one, that of dislocation of the vertebræ of the neck, without destruction of life. Sir Astley Cooper informs us that he has seen no such accident; though he does not wish to deny the *possibility* of its occurrence. Yet an instance is recorded by Boyer, who appears to have been a witness of it himself, and who speaks of this dislocation as an occurrence not

We may be asked, why the dislocation spoken of is so very rare, as not to have been seen even by these great practitioners? In reply to this question I would premise, that I do not consider myself bound to answer objections to a fact; it is enough to show its existence. However, I am ready to give all the satisfaction I am able, and therefore shall assign as the reason for the infrequency of this kind of dislocation, the following considerations and facts. The direction, in which the thigh bone is thrown out of its place, must depend principally on the direction in which the displacing power is applied. In order to dislocate the thigh upwards and backwards on the ilium, or above and without, force must be applied from below and within. Now in injuries accompanied with dislocation of the thigh, the force is most frequently applied to the knee on its outside and lower part, and the blow is received on the outside of the knee in consequence of the natural inclination of the thigh bone from without inwards, as it passes from the hip to the knee. If, for instance, a man falling from a scaffolding, strike the knee, it will usually be on its outer part. If there should be a consequent dislocation, the thigh bone will be pushed upwards and outwards on the ilium; and this is the reason why this dislocation is more common than any of the others, notwithstanding nature has in precisely this direction placed the strongest guard of bone, ligament, and muscle; no doubt because it is the part most exposed to this accident. While this is the most common mode of the application of force to the knee, there may be many others which will of consequence be calculated to give the bone various directions. If the force be applied below the knee and on its fore part, then, the thigh bone being thrown upwards till it tears the ligaments and muscles, its head will issue at the lower part of the ligament, and a slight inclination of the knee inwards will throw it in a direction to rest on or near the outer part of the socket, and in this way produce the dislocation backwards and downwards. It has also seemed to me probable, in the case of an individual falling with his horse, the foot being engaged under the horse, and the subject coming down on the side of the sacrum and ischium, that the weight of the animal received on the inside of the thigh, under such circumstances, would force the head of the bone outwards and downwards. The cause of the infrequency of the dislocation downwards and backwards, I therefore suppose to be the infrequency of the application of a sufficient power in the direction required to produce it.

unfrequent. "A lawyer writing at his desk, heard the door behind him open; he quickly turned round his head to see who was coming in, but could not bring it back to its natural position. Many surgeons of Paris have seen this patient; his head was turned to the right, and slightly inclined to the shoulder of the same side." Sir Astley's words on the subject are the following: "It has been generally said by surgeons, that dislocations of the spinal column frequently occur; but if luxation of the spine ever does happen, it is an injury which is extremely rare; as in the numerous instances which I have seen of violence done to the spine, I have never witnessed a separation of one vertebra from another, without fracture of the articular processes; or if those processes remain unbroken, without fracture through the bones of the vertebræ; still I would not be understood to deny the possibility of luxation of the cervical vertebræ." An instance of partial luxation of the cervical vertebræ successfully reduced, has, I have been told, recently happened in this country, but the circumstances have not been represented to me so exactly as to authorise me at this time to cite it. The repetition of quotations is not however necessary, I suppose, to support the general assertion made above; since few will deny that accidents have occurred, and do frequently occur, which the most practised surgeons may not have encountered. It is on the subject of injuries of the spine, that Sir A. Cooper has been recently attacked with great violence by a surgeon and author of high standing in London. I have alluded to this attack for the purpose of showing that the strongest claims on the good will of the profession and the public, have not exempted even Sir Astley Cooper from sarcasm and reproach.

Having thus assigned the reasons which might have influenced the opinions of Petit, Boyer, and Sir Astley Cooper; having removed in part, if not wholly, the objections which might have operated in the minds of the two last named surgeons; and having shown that there is not any physical obstruction to the occurrence of the accident in question; I might add that in this state of facts, the well authenticated and positive evidence of a single instance of its occurrence would be sufficient to satisfy an impartial mind. This could indeed be properly said of any such case, described by a perfectly disinterested practitioner; but, as I am now situated, such a fact would not, of itself, present sufficient grounds for confidence. Instead, therefore, of bringing forward my own experience in this stage of the argument, I shall beg leave to look into respectable authors for the purpose of ascertaining, whether the opinion formed by me is an insulated one, or whether it is supported by good authorities.

The authorities which I shall prefer in the present, as in a former case, are those which are considered most popular, and therefore best fitted to represent the general sentiment. The first which presents itself, in this light, is the article on the subject of dislocation in the Cyclopædia of Rees.*

Of dislocations of the hip, this author says, "These dislocations may take place upwards and outwards, on the external surface of the os ilium; upwards and forwards on the body of the os pubis; downwards and inwards on the foramen ovale; and *downwards* and *outwards on the os ischium.*"

"Luxation of the femur downwards and backwards, may, like that of the humerus inwards and forwards, be primary or secondary: it is primary when in consequence of some effort, the head of the femur is forced from the acetabulum, at its *inferior* and *posterior* parts, and is placed at the junction of the *ilium* and *ischium*;† it is secondary when it succeeds to the luxation upwards and outwards, the head of the femur, which was placed at first in the external iliac fossa, sliding downwards and backwards; its passage in this direction being favoured by bending the thigh on the pelvis.

"In these two cases, the head of the femur rests against that part of the ossa innominata *where the os ilium and ischium join;* the muscles which cover the posterior parts of the articulation, such as the Pyriformis, Gemini, Obturatores, and Quadratus, are raised up and stretched; the Psoas magnus and Iliacus internus are in a state of great tension, and this explains the turning of the limb *outward.* When this luxation is primary, the extremity is lengthened; a hard tumour is felt at the posterior and superior part of the thigh; the great trochanter, by descending, is removed further from the spine of the os ilium, and the knee and sole of the foot are turned *outward;* but if it be secondary, the thigh is much bent on the pelvis,

* The composition or preparation of the surgical articles in this work has been attributed to Mr. William Lawrence of St. Bartholomew's Hospital, a distinguished author, lecturer, and surgical practitioner in London. It must be noticed, that the titles of the different species of dislocation are a little varied by different authors; though not so much but that a moderate degree of attention will show their agreement with each other in meaning.

† The point of junction of the ilium and ischium might be thought too high a situation, when the head of the bone is there, to present a luxation downwards. To judge rightly of this, it is proper to consider that the upper part of the ischiatic notch is but little above the acetabulum, and that, when the head of the thigh bone is there, the limb is very little, sometimes not at all, shortened; that when the head is lodged at the junction of the ilium and ischium, it will be considerably lower, and the limb of course decidedly elongated; and when on the lower part of the ischiatic notch and spinous process of the ischium, it will be much elongated, that is, to about the same degree as in dislocation forwards and downwards, which is well known to be frequently equal to nearly three inches.

the knee and sole of the foot are turned inwards, because the primary luxation has been upwards and outwards. The secondary luxation in this direction is much more frequent than the primary."

Omit what relates to the secondary luxation, and you have in this description, as nearly as may be, the phenomena in the case of Mr. Charles Lowell, that is, of a dislocation "backwards and downwards, in or about the ischiatic notch;" for if I had drawn up a statement of the symptoms of the case, it could scarcely have applied with more accuracy than this; especially the account of the anatomy of the muscles in this accident, which, independently of this author, I had examined and verified in every particular. Will any one believe that imagination could have formed these symptoms in my mind, years before I had read the description quoted above?

In other histories of the case, we find some varieties of concomitant symptoms; but they are not greater than might be justly expected, on considering the different *degrees* of *laceration* of the capsular ligament of the joint, especially when the anatomy of all the parts concerned in this dislocation is rightly understood.

The Edinburgh Practice of Surgery contains the following account of the matter. "From the great strength of the hip joint, it was formerly believed, that the head of the thigh bone was never luxated by external violence. But it is now known that it happens by no means unfrequently."

Having described three other kinds of dislocation, the dislocation backwards and downwards is thus spoken of: "When the ball slips backwards and downwards, the leg is lengthened, the toes turned inwards, and the great trochanter is lower than that of the other limb. If the ball slips directly downwards, the toes keep nearly their natural situation." This last dislocation is apt to be converted into that backwards and downwards, for when the head of the os femoris slips out of its socket at the lowest part, there being no very convenient lodgment for it directly under the socket, it glides backwards, and in this way the dislocation backwards and downwards may sometimes be produced.

Among the more distinguished French authorities on this subject, we find the following: First, the Encyclopédie Méthodique, a work produced by the united science of the French. "We may," say they, "distinguish different luxations, according to the place the head of the bone occupies, out of its cavity; thence the different names of luxations upwards and outwards, upwards and inwards, *downwards* and *outwards*, downwards and inwards."

Richerand, author of the system of surgery called Nosographie Chirurgicale, also of the system of Physiology, and of a surgical work just published, which has excited great attention in France and England,* holds the language of Rees's Cyclopædia, or the account given by the one is a translation from the other. This, as we have seen, agrees more nearly with our case than any other history of this accident, and we must view in it the united opinions of Richerand, and of the editor of the surgical part of the Cyclopædia. I quote a few lines only to show this agreement. "The femur may be luxated from its articulation with the innominatum, to be carried upwards and outwards, on the exteral face of the os ilium; up

* This new work of Richerand's is written on the plan of Sharp's Critical Inquiry. It contains the most liberal and impartial view of French surgery, which has ever appeared. The criticisms on the writings and improvements of the French, English, and German surgeons, are some of them excessively severe, especially those on Desault, and Dupuytren, the great surgeon of the Hôtel Dieu; in some of which I should venture to disagree with this writer. However, it must be allowed that he shows great learning and independence, and it is probable his publication will produce considerable improvement in French surgery.

wards and inwards, on the body of the pubes; downwards and forwards, in the obturator foramen; *downwards* and *backwards*, on the os ischium."* He distinctly states in regard to the latter, that the limb is lengthened and turned outwards.

According to Leveille, author of the systematic work called "New Surgical Doctrine," "the femur may be displaced outwards and inwards, forwards and backwards." "The luxation backwards, in *posteriorem partem*, Hippocrates, Celsus; *downwards* and *outwards*, impossible, according to Petit; *behind* and *below*, according to Bertrandi; posteriorem et inferiorem, according to Callisen; posterior and inferior, Monteggia; is indicated by the *flexion on the pelvis* of the *thigh*, when in a state of adduction; by the *elongation* of the limb, the knee and point of the foot being directed inwards; by the great trochanter being lower in front, as well as the bend of the hip, flat above, broader below."† "The last variety of luxation, is with the capsule lacerated behind, with the head of the bone applied on the *tuberosity of the ischium, where it is retained by the tension of the superior part of the* capsule, which is not torn, and by the contraction of the muscles attached to the trochanter."

Let it be noticed, that the above passage gives the authorities of Hippocrates, among the Greeks; Celsus among the Latins; Bertrandi among the Italians; Callisen among the Swedes; and Monteggia among the Italians.

All in favour of the dislocations backwards, and most in support of that backwards and downwards, and each of them, at least of the modern authors, at the head of the profession in their respective countries; Callisen and Bertrandi, are, it is well known, surgeons and authors of the highest rank for learning and accuracy; they are, particularly the former, not to be surpassed, for their knowledge and correctness. They distinctly maintain the doctrine I have endeavoured to illustrate.

* Le femur peut se luxer dans son articulation avec l'os innominé, pour se porter en haut et en dehors, sur la face interne des os des îles, en haut et en dedans sur le corps du pubis, en bas et en avant sur le trou obturator, *en bas et en arrière sur l'os ischien.*

† It will be sufficiently obvious that all these authorities are not adduced because their description of symptoms accords exactly with those of the case in discussion. The principal object of them is to establish the disputed fact of dislocation backwards and downwards. The symptoms are described with some variation by different authors. They all agree that the limb is elongated. The direction of the foot is, according to some, outwards, to others, inwards, and again, according to others, straight forwards. These varieties, as I have explained, are produced by the different degrees of laceration of the capsule. When the head of the os femoris is at the lower part of the ischiatic notch and spinous process of the ischium, the foot will, I apprehend, rather incline to turn outwards than inwards, in most instances.

[*The balance, including the Appendix, will be given in the next number.*]

[*With a view to have Dr. Warren's letter uniform, we continue the paging from our last number.*]

The only additional authority I shall think necessary to cite from the French, is that of Delpech, the learned Professor of Surgery in the University of Montpelier. His remarks are very comprehensive, and exhibit great observation and a profound knowledge of the subject:

"The exact analysis of facts, demonstrates that we must refer every luxation of the femur, as to the direction of the displacement, to four different species. In the first, the head of the bone passes towards the external iliac fossa; in the second, it is placed on the horizontal branch of the os pubis; in the third, it rests on the foramen ovale; in the fourth, the femur is supported between the cotyloid cavity and the great ischiatic notch. It is probable that the accidental relations established by the luxation, do not always take place exactly between the same points; observation demonstrates, on the contrary, that there are *some variations* in these particulars; but we may be assured from facts, that the articular capsule is always ruptured towards one of the points indicated.

"We have given to each of these species names taken from the position of the displaced bone: thus in the order in which we have enumerated them, the first is called *posterior superior;* the fourth *posterior inferior;* the second anterior superior; and the third anterior inferior. It would be more simple to call them superior, inferior, anterior, posterior."*

He then proceeds to show that the respective frequency of these four species depends not so much on the anatomical structure, as on the direction of the force applied; for the socket is most elevated at its upper part, and yet dislocations are most frequent in this direction. The luxation on the ilium upwards, is, he thinks, the most frequent; next downwards and forwards; third, upwards and forwards; and in regard to the last species he says: "As to the displacement, by means of which the femur is carried *near to the ischiatic notch;* it has been observed so seldom, that *respectable physicians have even denied its possibility.* It is however *incontestible* that it has occurred; but it is the most rare of all. Some have attributed this last kind of luxation to a secondary displacement, which might succeed that, in which the head is lodged in the *iliac fossa;* but there is nothing to prove the exactness of this assertion.

"Some writers have thought that certain luxations of the thigh might change their direction, by the effect of a consecutive displacement; it has been thought, for example, that the luxation backwards and downwards never took place immediately, but that it might be the remote result of that upwards and outwards, by means of a subsequent flexion of the thigh. But in the first place, the immediate luxation *backwards and downwards, can no longer be a subject of dispute in the present state of science;* in the second place, observation has nowise proved this transformation from one luxation to the other; finally, the state of the muscles in the first species seems to render the secondary displacement, of which we speak, a thing impossible."†

These quotations will be closed with two authorities, published in our

* The last is the nomenclature adopted by Sir A. Cooper.

† Précis Elémentaire des Maladies Réputées Chirurgicales; Par J. Delpech. Paris, 1816. pp. 107, 8, 9, and 17.

own country; one contained in the surgical system of the late Dr. Dorsey, and adopted by him from the French; the other, from a work recently translated and published by Dr. Godman.

"The dislocation of the hip may take place," says Dr. Dorsey, "upwards and backwards," (or upwards and outwards) "on the dorsum ilii; secondly, upwards and forwards, on the os pubis; thirdly, downwards and inwards, into the foramen thyroideum; fourthly, downwards and *backwards* on the *os ischium.*"

The name of Lisfranc, and his improvements in surgery, have lately excited attention in Europe and this country. Many of these improvements are indeed merely changes, but others exhibit great ingenuity, and give him a fair title to the respect of the profession. A number of his operations I have practised at the Massachusetts General Hospital, with much satisfaction to myself, and I trust with benefit to my patients. An account of his practice has been published in Europe by Dr. Costar, which that ingenious naturalist of Philadelphia, Dr. J. R. Godman, has just given to this country, with some notes of his own. The work of Lisfranc must be considered as the latest statement of surgical practice and opinions, having been published since Sir Astley Cooper's third edition. It contains the following account of the different species of dislocation of the hip joint.

"*Luxations of the Femur.*—This luxation may occur in four different directions: 1st, upwards and outwards; 2d, upwards and forwards; 3d, downwards and inwards; 4th, *downwards and backwards.*

"John Lewis Petit published his opinion on this subject in France one hundred years ago, Boyer about fifteen or twenty years, and Sir Astley Cooper in England, about ten. Yet, notwithstanding these remarkable exceptions to the public voice, the general opinion evidently remains unchanged, and, as the distinguished professor of Montpelier remarks, 'the *immediate luxation downwards and backwards can no longer be a subject of dispute in the present state of science.*'"

Such are the authorities in support of the existence of the kind of dislocation I had presumed to exist in the case of Mr. Lowell. If my presumption in this instance be charged with the epithets "absurd," "unaccountable," or "incredible," it will be seen that I must have the honour of participating in these charges with a large part of surgical authors.*

If, however, there be no absurdity in believing the possibility of this accident, I may be allowed to state the special reasons for adopting such an opinion in this instance; to support this opinion by particular facts, which have come within my own cognizance and that of other practitioners; and, further, I may be permitted to obviate some difficulties which have been suggested, and reply to some remarks which have been made on my opinion.

No one will suppose that I examined all the authorities introduced, at the time the case of the plaintiff was submitted to my investigation. His case appeared to me a difficult one indeed, at first view, but I was not wholly without knowledge of the opinions of authors concerning it, and therefore, after one inspection of the patient, thought it sufficient to refer to the treatise of Sir Astley Cooper, a copy given me by the distinguished author, and which I had annually referred to, quoted, and sometimes exhibited in my surgical lectures, explaining by its engravings the distinctions between these dislocations. The work of Sir Astley did not afford support to the opinion of the case, which on the whole seemed to me most tenable; neither did it,

* I do not wish to be considered, in thus dilating on this topic, to be very anxious to convince every one of the species of luxation. The difficulty of doing so is, I am sensible, very considerable. What seemed principally desirable was to establish the *existence* of a dislocation, and to assign a sufficient number of facts and authorities to show there was no "absurdity" in my opinion of the species.

to my apprehension, declare such a case impossible, and I had authorities on the other side; and, what was of more weight, the evidence of my own senses. True it was, that the phenomena much resembled those of another, and more common dislocation; that, namely, which happens downwards and forwards into the foramen ovale. In this state of doubt, I looked for the sign, which must in every case be considered pathognomic, or truly characteristic of the nature of the accident; that is, the situation of the head of the displaced bone. If I could find this, whatever might be the other appearances, the species of the dislocation would be satisfactorily made out. Let us suppose, as a parallel example, a dislocation of the shoulder, the first aspect of which might lead the surgeon to the belief of a luxation in the arm-pit; but that on passing the hand over the shoulder, the head of the bone should be felt on the scapula. Would his opinion halt after he had made this discovery? The situation of the head of the bone is an unfailing indication of the species of dislocation. I considered it so, my opinion was formed thereon, and subsequent events served to corroborate it; for when I examined the patient, while under the action of the pullies, I had the perception of its *motion*, and pointed it out to those who stood around me. The authority of books should not, I think, be opposed to such evidence.*

The hon. Judge informs us, however, that "in this dislocation, the head of the thigh bone is described as being buried so deep, that it cannot be felt distinctly except in very thin persons."† He did not, perhaps, recollect, that Sir Astley Cooper was speaking of a case entirely different from Mr. Lowell's, when he said, that "the head of the bone could be discovered only in very thin persons." Sir Astley was alluding to dislocations with a *shortened* limb; whereas in this the limb was *lengthened*. Sir Astley alluded to those in which the limb is turned in; in this it was not turned in. Sir Astley referred to cases in which the head of the bone was above the socket; in this it was below it. In fine, Sir Astley described the luxation in which the head of the os femoris is imbedded in that deep hole at the upper part of the notch, while in this, it was from one to two inches lower, on the ischium, or the inferior part of the ischiatic notch. We, in fact, spoke of things quite different, and although it were true, that in no case could the bone be felt in the upper ischiatic or *iliac* notch, it would not in the slightest degree go to prove that it might not be so in the lower and superficial part of the notch, or on the adjoining os ischium. It is found in the latter case at the

* But is it not possible, that I might have been deceived? Undoubtedly. The evidence of the senses is sometimes fallacious, and what seemed to me to be the femur, might have been something different. The Hon. Judge expresses his conviction that this was the case; and the learned attorney is perfectly certain; he "knows" it was not the head of the os femoris. It might have been a wen, an exostosis, a hernia, a piece of the socket, or some non-descript tumour. These are possibilities, I admit, and I must admit that my senses might have deceived me in the whole of this business. But as nature has bestowed no other medium of communication with external objects, I must continue to trust the evidence of these senses, weak and erring as they may be.

† "Now the plaintiff," he adds, "has been before you, and you will judge, whether he is to be considered *a very thin person*." I must ask, whether it was altogether just and fair to call on the jury to take the appearance of the plaintiff nearly three years after his accident, as a specimen of what it was, when he had just issued from his confinement, and was hobbling with difficulty on crutches. The fact is, that his appearance was essentially altered in the interval. He had added most materially to his bulk; and the prominences of the bones were, at the time of the trial, well covered with adipose substance. He then indeed exhibited the aspect of full health and vigor, and bore none of the phenomena, adapted to excite the compassion of the court and jury. When I saw him, the case was different. His appearance was not sickly, neither was he charged with a superabundance of fat. He exhibited muscle and bone in such distinctness, that all parts, naturally prominent, might be made out by a careful observer. I object, therefore, to the presentation of Mr. Charles Lowell, in the month of June 1824, as a specimen of what he was in 1821.

upper part of the thigh. There it is described as being sensible to the touch by Richerand, and there it was that I perceived it in the case of Mr. Charles Lowell.

Another exception must be taken to the opinion expressed so forcibly by the hon. Judge.* He appears evidently to have examined the book of Sir Astley Cooper, from his having made the above mentioned reference to it, and moreover states, that, "Page 79" in that book (intending page 68,) "gives an example of a genuine dislocation into the ischiatic notch, in the case of a young man admitted into Guy's Hospital, under the care of Mr. Lucas." We must suppose, therefore, that he had looked at page 68, and read the account of the case.

But it appears that, in this very case, the head of the thigh bone was perceivable, and *was actually perceived* by the examining surgeon. Yet this patient is not said to have been "a very thin person." At page 79, however, we have, in Case 5th of dislocations into the ischiatic notch, a more distinct account of the condition of the patient. "James Hodgson, a sailor, *a strong, muscular man*, was admitted into St. Thomas' Hospital, &c."—After stating the appearances and consequent symptoms, he proceeds thus: "Monday evening, the 14th, the swelling had greatly subsided, and I thought I could now feel *the head of the bone*, on rotation of the limb." "Mr. Cooper at my request very kindly saw it in the evening, and immediately declared it to be a dislocation into the ischiatic notch, and upon his rotating the thigh, I could, much more distinctly than before, *feel the head of the bone in the ischiatic notch.*" The reduction was attempted, and in the course of it, "we kept the extension up," says he, "about ten or twelve minutes before we used the strap to raise the head of the bone, and until I thought *it had made some progress* towards the acetabulum. We then continued the extension, gradually increasing it, and at the same time endeavouring to raise the head of the bone, and turning the knee outwards, for about 15 minutes. I had now *lost the head of the bone*" The extension was continued some time, and on removing the pullies, the limb was found to be reduced. In this case, the head of the bone was repeatedly observed, and under circumstances resembling those of our case, with this difference, that in that here related, the head of the bone lay much deeper than in ours; yet it was perceived, though the patient was a "strong, muscular man."

Case second of this same dislocation, is the account of what befel Wm. Dawson, aged 34, in a quarrel. His hip was dislocated, and much swelling followed. "On Sunday, 30th of August," says the narrator, "we accordingly met, which was fifteen days after the accident, and from the complete removal of all swelling, the whole femoral bone was *satisfactorily traced to its rounded head, which was lodged in the ischiatic notch.*" Whether Dawson was thin does not appear. It seems probable that he was not "a very thin person," being in the prime of life, and having met with the accident, whilst rioting and quarrelling with his companions at harvest home.

Thus, in three out of five cases of this accident mentioned by Sir Astley, the head of the os femoris was distinctly felt; in the fourth it is not noticed whether it was felt or not, and in only one of the five is it positively stated that it could not be felt; and it must be recollected, that in all these cases the head was more deeply imbedded, than in the dislocation downwards.

I feel myself called on to remark further, that the bench on this occasion highly eulogized the skill, success, and wide range of practice of the deponent A., evidently for the purpose of quoting with more effect the opinion

* In thus speaking of a judgment emanating from so high an authority, I wish to be considered as intending to do it with all the respect consisting with the development of truth. The person and character of the honourable gentleman who sat on the bench, and who has done me the honour of using my name, are as much unknown to me as if he were an inhabitant of Great Britain; but I respect the office.

of the deponent, that in this case there did not exist any dislocation of the thigh bone. Now I would not at any time be understood to call in question the justice of that eulogy, and least of all at this part of my argument, since it so happens, that this deponent has given a distinct and positive opinion, as to the point we are considering; for in his deposition he says, "Both when the head of the bone is on the back of the hip bone, and when in the ischiatic notch, *the head of the bone can be distinctly felt by the hand.*" How is it that the bench should have so much regarded the opinion of this deponent, when opposed to mine, and yet passed it altogether without notice, when it happened to agree with it? On the contrary, my testimony, even when thus supported, is referred to with obvious marks of distrust; for after having stated that the head of the bone can be felt only "in very thin persons," and requested the jury to consider whether the plaintiff were "a very thin person," and whether his were one of those cases, therefore, "in which it would be *quite easy* to discover the head of the bone," he adds, "Dr. Warren, and the other physicians in Boston, however, declare that the head of the bone was distinctly felt."*

"The genuine dislocation into the ischiatic notch," referred to in the work of Sir A. Cooper by the hon. Judge, is adduced for the purpose of showing that in this case the limb is not lengthened, but rather shortened, and the knee and foot turned inwards; "while," says he, "Lowell's are represented as turning outwards." Here again I must remark, that the case described by Sir A. Cooper is a different one from that of Mr. Lowell. The relations of the head of the os femoris, and consequently of the rest of the limb, vary in regard to two essential points: 1st, as to the acetabulum; 2d, as to the os innominatum. In the dislocation described in Sir A. Cooper's work, the head of the bone is in the superior ischiatic notch, which is *above* and much behind the acetabulum, and the limb must in such a case be shortened, if its length be in any way altered. In Mr. Lowell's case, the head of the bone was *below* the socket, and the limb must of course have been longer than the other. Next, as to the relation between the os femoris and os innominatum. In Sir A. Cooper's case, the head of the bone, gliding backwards to the superior ischiatic foramen, would be inevitably turned over, from its inability to draw the whole limb so far back. The result of this would be, that the trochanter would be rolled forwards, as seen in Plate II, and with it the whole limb, the knee and foot being necessarily turned in. But in the dislocation downwards, the state of things is very different. Here the head of the bone passes backwards a little space only, and the change of place is much greater downwards than backwards. The limb does not revolve, or if in any degree, but slightly, and that in the direction backwards instead of forwards; for its head is supported against the os ischium, as exhibited in Plate III. In this same plate we have an opportunity of comparing the direction of the bone in this case, with its direction in that of the dislocation upwards and backwards; we see at once the great difference between the relations of the head to the acetabulum

* As the Hon. Judge seems to hold our positive testimony on this point at a low rate, I feel myself bound to declare, and I hope it will not be imputed to improper motives that I do so, that there are few points of the surgical art, to which I have been called to give more attention, than those which relate to affections of this part of the body. Diseases and injuries of the hip are among the most frequent subjects of the skill of the surgeon, and during a period of twenty years I have had, to use the words of a high authority, "no inconsiderable share of experience in these cases;" having rarely been without some instances of them under my hands, and often many at the same time, presenting great varieties of appearance, and calling for the exercise of the most exact discrimination of the eminences and depressions, with a view to determine questions of treatment, in which the health, comfort, and life of the patient were involved. Of course, I was not altogether without the practice required to discover the condition of parts about the hip joint, and to determine, whether there existed changes from the healthy structure.

and os innominatum in the two cases; and may infer the concomitant variation in the positions of the limb.

If we compare the two representations of the os femoris in Plate III, with the representation in Plate II, we may understand its relations with the acetabulum and os innominatum in the three different cases of dislocation backwards. At *a*, Plate III, we see the head of the bone, as in the dislocation upwards and backwards on the Dorsum ilii. The limb is much shortened, the knee and foot turned much inwards. In Plate II, it is seen as in the dislocation backwards into the superior ischiatic notch. The limb is but little shortened, and the knee and foot turned slightly inwards. In Plate III, *b*, we see the dislocation backwards and downwards. The head of the bone is on the os ischium, the limb is much lengthened, while the knee and foot are turned outwards.*

These are the representations of facts; they are founded on the anatomy of the parts concerned, and supported by the surgical observations already detailed. I have demonstrated these facts to a class of more than a hundred students of medicine and surgery, and exhibited to them the manner in which the dislocation would be produced, and the position which would be given to the limb, in the dislocation downwards, by the prominence of the socket bone, the lacerated ligaments, and overstretched muscles. I showed that, in this case, the limb would be very considerably lengthened; that the foot and knee might stand forwards, or turn slightly outwards; and that, above the trochanter, there would be a deep depression, corresponding with the region of the socket. Moreover it was demonstrated, that the head of the bone, when forced from its socket in a direction downwards and backwards, lacerating the capsular ligament, passes below the Pyramidalis muscle, raises the Gemelli and Obturator internus, and, to a certain extent, takes their places; that sometimes the muscles may be ruptured, but that ordinarily they would remain in a state of tension; that the Obturator externus and Quadratus would be nearly in the same condition; that the Pectineus and the strong muscles, Psoas magnus, Iliacus internus, and short head of the Triceps, would be violently extended, and, acting on the bone from without inwards, would press it against the bulge of the socket, and thus fix it firmly in its new position; that the anterior part of the Gluteus medius would be greatly strained, and would draw the thigh bone upwards, were it not resisted by the prominence of the socket, together with the action of other muscles.

Such a view of the anatomy of the parts concerned must, I think, satisfactorily show the possibility of the existence of a dislocation backwards and downwards, and the accordance of the phenomena indicated with those of our patient in points essential. The state of rotation may seem to present an exception to this remark; but it has been shown already, that this condition may be varied by circumstances, in the same manner as happens in regard to fracture of the neck of this bone, in which case, though the limb is usually turned out, it sometimes turns in. This opinion will be confirmed, on considering that the round head of the os femoris is sometimes resting on the plain surface of the ischium; and that the position assumed by it will be influenced by the degree and direction of the dislocating force, as well as the subsequent condition of the capsular ligament. The action of the muscles, too, while it is always to be kept in view, is not

* To Dr. Flagg I am indebted for these views of the different dislocations. Having myself examined the phenomena in a number of instances, I procured a subject, and requested this gentleman, in whose anatomical and mechanical skill I had great confidence, to examine and represent the parts in the dislocation in question, and to ascertain how far they agreed with my views. Dr. Flagg made the examination by himself; and I had the satisfaction to find that the result confirmed me in the impressions received, and the opinions formed thereon.

to be considered in an insulated way, but in connexion with the situation of the bones and ligaments. For the muscles that tend to draw a bone in any given direction, may actually prevent it from proceeding in that direction, by pressing it against a ligament, or another bone.

To say that the head of the bone in the case in question was situated on the os ischium, may, on a superficial view, be considered as a contradiction of my former opinion. It is not so, however; this opinion was, that the head of the bone was lodged in or about the ischiatic notch, and this was as near the fact, as I could properly speak. From a survey of the anatomy of the parts since, I should be disposed to believe that the situation of the head of the bone was probably at the lower part of the ischiatic notch, on the plain surface in front of the spinous process of the ischium. It is easy to quibble about the term "ischiatic notch," but I dispute not for words, but facts. I said that the dislocation was of a particular species, and distinguished that from what may be called a sub-species, by the term *downwards*. If this last expression had not been used, I might have appeared to confound it with the dislocation backwards. Whether it was in, or near, the ischiatic notch, however, it was equally a dislocation belonging to this one of the four species commonly admitted, and generally designated by the terms I used.

The manner in which the injury is said to have been received, seems to confirm this opinion of the nature of the case. It appears that Mr. Lowell fell on his back, inclining a little to the left side. Probably he struck on the ground with the os sacrum and left ischium, receiving at the same instant the weight of the horse on the inside of the thigh. The knee, and perhaps the foot, were in consequence fixed to the ground by the same weight. In this situation, if the bone were driven backwards out of its place, by the pressure on the inside of the thigh, the head of it would be carried downwards and outwards, in consequence of the confinement of the foot or knee. Such at least was the impression conveyed to me by the description of the patient, at a time when he could have no possible inducement to misrepresent the circumstances. Such, or nearly such, also, is that conveyed by Reynolds' deposition, for there is a slight degree of obscurity about this testimony, arising probably from the confusion incident to the circumstances of such accidents.

The deponent A. intimates that a different impression was given to him, by the plaintiff's description; the impression, namely, that he fell on his hip, from which he concludes that the bones were broken and not dislocated.—He reasoned, therefore, as before observed, from erroneous data.

That a dislocation, and what is more to the purpose, an ischiatic dislocation, might occur in circumstances similar to those of Lowell's accident, is proved by a case related by Sir Astley Cooper. It is that of "John Norgott, aged 40, who was carried into Winchester hospital twelve days after his accident. He reported that his horse had fallen with him, and on him, so that *one leg was under the horse*, while his body was in a half bent position, leaning against a bank." The injury proved to be a dislocation into the superior ischiatic notch, which was reduced and the patient cured. It may be said that a dislocation backwards, in this way, gives no support to the probability of a dislocation backwards and downwards, from a force applied in a similar way. I am of opinion however, that it does afford support to it. It is obvious that, if it should happen that the knee or foot were caught under the horse, and the limb were in these circumstances dislocated backwards, as in Norgott's case, it would be more likely to be driven downwards and backwards, than directly backwards; because the head of the bone in such a case would be compelled to describe the arc of a circle, instead of moving in a direct line backward from the socket, and this I suppose to have happened in Lowell's case.

This opinion is confirmed by the following account of a dislocation down-

wards, produced by an application of force in the same direction as in the case of Mr. Lowell. "M. Ollivier of Angers, in the third volume of the General Archives of Medicine, has described a luxation of the femur directly downwards, observed by himself. This "was produced by the stroke of a large branch of a tree, which in falling struck the internal and inferior part of the thigh of a man who had fallen to the ground, and pushed it violently in abduction," that is, outwards.

This accident demonstrates that a force applied on the inside of the thigh is adequate to producing a dislocation downwards; while Norgott's case shows, that a force in a similar direction may produce a dislocation backwards; certainly therefore a force so applied may be supposed adequate to produce it in an intermediate direction, or downwards and backwards, as happened in the case of Mr. Lowell.

"The signs observed," in Mr. Ollivier's case, "were these; a slight flexion of the thigh on the pelvis with abduction, and a feeble rotation of the limb inwards. The thigh was a little flexed. There was no sensible elongation of the thigh, on account of the *oblique* direction of the luxated bone. The bend of the groin was deeper than that of the opposite side, without however a great depression; the great trochanter was situated lower than in the natural state, and the hip rounded, and more prominent than that of the opposite side, especially when the patient was examined by placing him at the foot of a bed, after having made him bend both thighs; it did not form folds inferiorly with the thigh. The head of the bone was not distinctly felt; extension of the thigh on the pelvis was impossible; motions of adduction could be slightly executed, though with pain; abduction was possible to a greater extent; the patient when lying on the back kept the limb slightly flexed."

In this case the limb is said not to have been elongated, on account of its obliquity. By this is meant, probably, that the limb did not appear elongated, because it could not be straightened; for as the head of the thigh bone was directly below the socket, the thigh must have extended lower than the other, had the two been placed side by side.

Further it appeared from this case, that, when the head of the os femoris rested below the socket, there was a feeble rotation inwards. From this fact we have a right to infer that, if the head of the bone had glided backwards to the ischiatic notch, and its spinous process, the rotation would have been outwards, necessarily, in consequence of the inclination of that part of the ischium.

In another part of the description quoted above, we find the following passage. "We must, however, make the remark, that, in the different luxations of the thigh, we observe, in different subjects, slight differences in the relations of the head of the thigh bone, and in the lesions of the neighbouring parts; differences which probably depend on the greater or less degree of violence with which the displacement has been effected, and on the greater or less time since it had taken place."*

There remains only one species of proof applicable to such a case, which has not been exhibited. Sir Astley Cooper has elucidated and confirmed many of his accounts of dislocations, by descriptions of the actual appearances of the injured parts, as seen in the dead body. Evidence of this kind indeed must afford incontrovertible proof of the existence of particular species of dislocation, and if we could discover a morbid specimen, or the description of one, exhibiting the head of the bone displaced in a direction backwards and downwards, it would be a complete demonstration of the point in question. But the concurrence of circumstances necessary to the existence of such a preparation, would be more than could reasonably be ex-

* These quotations are from the "Dictionnaire de Médecine," T. XIII. Article "Luxation."

pected in so rare a case. To produce this, we must have, 1st, a case, such as was never seen by Petit, Boyer, or Sir Astley Cooper; 2dly, the bone must have remained unreduced; 3dly, the patient must have died; 4thly, his body must have been examined; 5thly, the surgeon must have known and distinguished the case; and, 6thly, he must have described it for the benefit of the world.

Such a concurrence of circumstances, I repeat, is hardly to be expected; and yet it has actually existed, and affords me the means of supplying the remaining link in this long chain of facts. There is a case in which the head of the os femoris is so situated as to demonstrate the dislocation downwards and backwards to our senses, a case in which the bones are no longer covered with flesh, nor their relations in any respect obscure. This preparation exists in the collection of Professor Du Pui, of the University of Leyden, one of the most scientific surgeons of the present day. It was observed and prepared by himself. Dr. Dylius, in his treatise "de Claudicatione," speaks of it in the following terms: "As to the fourth species of dislocation, on the os ischium, no specimen is given on either side of the body in a well-formed acetabulum, so far as I know, but if any one regard the remarkable inclination of the pelvis, certainly he cannot doubt the possibility of the existence of this species. Assuredly the head of the bone may slip to this part, as there is certainly a recent and excellent specimen in the collection of the celebrated Du Pui, where the capsule of the joint being relaxed, the head of the femur is placed, not indeed on the os ischium, but in its vicinity, on the sacro-sciatic ligament."* Thus it seems that the bone is, in this morbid specimen, placed on the sacro-sciatic ligament. But the place where I supposed the head of the os femoris to be lodged in the case which we have been considering, is the plain surface of the os ischium, in front of the spinous process of this bone, marked *F* in Plate I. Now the ligament, referred to by Dylius, is fixed to this process, and extends backwards and rather downwards, as will appear from considering the inclination of the process. The head of the os femoris, placed on the sacro-sciatic ligament, would therefore be situated lower than the point indicated by me as the situation of it in this accident, and would produce an appearance more strongly marked than in the case of Mr. Lowell. This morbid specimen therefore must be considered as conclusive and incontrovertible evidence of the actual existence of such a dislocation, as I pronounced to exist in this case.

Since the preceding remarks were written, another observation has fallen under my notice, which presents a case of the dislocation backwards and downwards with a more full and precise view of the condition of the parts concerned.

"The luxation on the ischium, or the ischiatic fossa," says the writer, "is often more difficult to recognise than the former." After assigning, as the reason of this difficulty, the want of shortening of the limb, he proceeds to quote, from Sir Astley Cooper, the symptoms of dislocation *in the ischiatic notch*, and then relates the following case with the subsequent examination and dissection, as an instance of this dislocation.

"In a man affected with this luxation and observed at Angers by M. Billard, the great trochanter formed a pretty strong projection on the outside, and a little forwards of the bend of the groin; the hip, slightly depressed

* "Quartæ tandem speciei, in Ischii osse, quantum scio, ad utramque latus, acetabulo saltem perfecte formato, nulla datur species, sed si quis pelvis modo memoratam inclinationem animadvertit, dubitare certe de hujus speciei possibilitate omnino nequit. Potest certe huc versum elabi femoris os, quale recens egregiumque certe exemplum servat in sua collectione celeberrimus Du Pui, ubi laxata articuli capsula, non quidem ad os ischium, sed in ejus vicinia, ad ligamentum sacro-sciaticum locatur femoris caput."

above and within, was prominent without and below. This wounded man having died two hours after the accident, on dissection we found the head of the femur situated in front of the ischiatic notch, *applied on the external part of the spinous process of the ischium; it had passed below the united tendons of the Pyramidalis and Obturator internus muscles, which obliquely crossed the neck of the bone, by passing above it; the two muscles Gemelli were torn, and the Quadratus raised and strongly extended. The cotyloid cavity was partly covered by the Iliacus and Psoas muscles, the contorted fibres of which were strongly extended, as well as those of the muscles Pectineus, Obturator externus, and short head of the Triceps.*"*

The agreement of this statement of the anatomy of the accident is so accordant in every respect with what I have already represented to be the actual situation of the parts, that I should scarcely have ventured to print this, after my own account, had not the sheets, containing my views of the matter, been already out of my possession, and in the hands of the publisher. This observation of Billard is sufficient, of itself, to overthrow all the reasoning set up on the presumption of the impossibility of such an accident.

It is only necessary to notice, in conclusion, that this case is found in a studied article on luxations, contained in a scientific work, intended for general circulation, and resting its claims to public attention upon the fulfilment of the pledge, given in its Prospectus, that it shall be "as complete as possible," and that "it shall present a faithful representation of the present state of the science on every subject which it discusses, without neglecting any fact, or even any opinion concerning it." In such a work we find a case of dislocation recorded, in which the head of the femur is found, on dissection, to be as nearly as possible in the situation in which I supposed it to be in Mr. Lowell's case; and the writer, moreover, agrees as exactly with me in the name, as in the place, of dislocation. He calls it "*a dislocation in the ischiatic notch,*" (luxation dans la fosse ischiatique); and, what is more, he does this with Sir Astley Cooper's book before him.

Remarks on some parts of the Charge from the Bench, and the Plea of the Attorney for the Defendant.

Not without great reluctance do I feel myself called on to advert, in this place, to some statements contained in the speeches mentioned above, which from their description, could not be noticed before, without interrupting the train of facts. We must first advert to the peculiar manner in which the Honourable Bench seems to have employed a certain review of Sir Astley Cooper's book, in the New England Medical Journal. It appears that this review contains sentiments favourable to Sir Astley Cooper; "and that the publishers particularly extract the results of Sir Astley Cooper's observations in regard to dislocations of the hip;" and these observations are quoted to refute the doctrine advanced by me. Thus far it is well. But the Judge would also make me responsible for the opinions of this review, or, in other words, he would employ it to make me contradict my own opinion, and confute myself. On what grounds is this severe judgment rendered to me? 1st, because the "*Massachusetts* Medical Journal" is "issued, if not avowedly under the sanction, yet supported by the physicians attached to the Massachusetts General Hospital." 2dly, because the number in which the

* This observation is found in a volume of the Dictionary of Medicine already quoted. The authors of the surgical part of the volume are Cloquet, Marjolin, Roux, and Murat; all of them anatomists and surgeons of the first rank in France.

review appeared, contains an article "furnished by Dr. John C. Warren." 3dly, "that it is followed by an *account* of an operation, probably that to which the patient submitted, for a dislocation into the ischiatic notch." Can any of these reasons prove me to be the author of the review in question? As to the first, it should have been known that, of eight consulting physicians, and two attending the Massachusetts General Hospital, only the two last, Dr. Jackson and myself, ever had any connection with the New England Journal. Of course this publication cannot be considered as sanctioned by the physicians of the Massachusetts General Hospital. 2dly, the fact of my name being attached to one article does not prove that I was an editor of the publication, since every body knows that such articles are furnished by the profession generally, and that the addition of the name to an article is considered evidence of its not being *editorial*. 3dly, it would puzzle a superficial inquirer to ascertain what is meant by the "account of the operation, probably that to which the patient submitted, for a dislocation in the ischiatic notch." On a little research, however, it will appear, that this *account* of an operation is an item in a *list* of operations, printed at the latter part of the said number, in the form of an advertisement, from the Medical School in Boston, to medical students. Such are the grounds on which I am made to adopt opinions thought to contradict those I had before given. The honourable Judge perhaps confounded three bodies of men, which are perfectly distinct from each other, namely, the physicians and surgeons of the Massachusetts General Hospital, the professors of the Medical School of Harvard University, and the editors of the New England Journal of Medicine and Surgery. The truth is, that though I was one of those who proposed the establishment of the New England Journal of Medicine, and was for many years an active editor; yet at the time of the said publication, I was not so, and never saw one article of that number of the Journal, as editor. But even if we were to allow the full force of his argument, and admit that I had been an editor of the New England Journal at that time, was there a propriety in making me responsible for the opinions of an article, standing with this title glaring at his head—"*From the London Medical Review*," a book printed on the other side of the Atlantic?

The honourable Judge throws out an intimation, that the case of Mr. Lowell has the signs of a dislocation in the foramen ovale. Can this be made to accord with the very decisive opinion of deponent A, whose character and skill the Judge has so greatly praised? This deponent denies the existence of *any* dislocation, and he is supported by all the depositions from physicians on that side. The Judge does not appear to have examined the patient, but founds this suspicion of his on the general resemblance of the symptoms to those of the supposed species of dislocation. The honourable Judge may be in the right; yet it must be remembered that his notion, not only is not supported by any direct examination of his own, or that of any medical person, but that it is in opposition to all the direct testimony in the case, from every quarter. Some of the deponents on the negative side have shown disposition enough to counteract the evidence given by those who first examined the case. If they could have discovered sufficient foundation for maintaining the doctrine of the dislocation forwards, is it not to be presumed they would have done it? or with the dispositions manifested, would not some of them have done so, with a view to increase the confusion and uncertainty of this case? and, if any of them who saw the patient had this opinion, could they be justified in concealing it, or rather were they not very criminal in doing so? The Judge appears to have been much influenced in forming his judgment, by a consideration of the manner in which the patient came by his misfortune. I have already remarked that reasoning on such sort of evidence is apt to be fallacious, on account of the great uncertainty as to the precise way in which accidents of this kind take place. I apprehend he may have been led to this opinion

by misapprehending the fact on which he builds. "The thighs," he says, "were widely separated when the original injury was received, and it is from this cause, as Sir Astley Cooper states, that this species of dislocation arises." I cannot find it recorded, in the deposition of the only man who saw and gave testimony of the events of the case, that the thighs were widely separated,* nor was it so represented by the plaintiff. Has not the Judge mistaken an expression, found in *Sir Astley Cooper's book* on dislocations, for one supposed to be contained in the deposition?

In order, apparently, to convey to the jury more forcibly the impression of the errors into which we had fallen, the Judge, while he professes that "this is a point to which he would be understood to speak with less confidence, as the supposition is without the direct support of professional testimony," adds at the same moment, "If it was a dislocation of this kind, the mode of reduction is very different from that which is required in cases of dislocation into the ischiatic notch." True, it is so. In a desperate case like this, however, it is presumable to those, who are willing to look on both sides, that the surgeon would vary the application of his powers in every possible manner, before he abandoned his patient. We have already intimated that, in this unscientific way of operating, as is well known to all practitioners, many bad dislocations have been reduced, as well by systematic as by irregular surgeons.

Finally, we must notice these expressions. "Notwithstanding," says the Honourable Judge, "the failure of this experiment, however, these gentlemen *persist* in their original opinion, and depose to that effect in this case." Astonishing! that we should *persist* in our original opinion, because we had failed in reducing a dislocation which we did not expect to reduce! But I forbear any further remarks. I would not hazard the imputation of a wish to diminish the respect due to a Judge of the Supreme Court; especially as I have not the least suspicion of the existence of any improper intention. It is obvious from his own acknowledgment, "that he had expressed himself somewhat strongly;" that the feelings of this gentleman had, from some cause or other, been drawn too much to one side of the case, so that it was not easy for him to see all the facts through the clear medium of a perfectly impartial and dispassionate judgment. I should not have ventured to say so much, had I not been compelled to do so by the respect I owe to public opinion, as well as by a sense of duty to my colleagues, the consulting physicians of the hospital, which, under actual circumstances, appears to me paramount to the other considerations alluded to.

In the course of the remarks made by the attorney for the defendant, we find a frequent recurrence of the terms, "Boston Faculty," "Boston Doctors," "Boston School;" and a "Boston Jury" is introduced in order to impugn their impartiality. Even the public institutions of this place and its vicinity are alluded to in terms of derision. "Is Harvard," says the attorney, "a faculty of *infallibles*, or the Massachusetts, a hospital of *incurables*;" and again, "bring down upon him the whole weight of the Harvard Medical Faculty, piled upon the Massachusetts General Hospital." Such attempts to effect an object of individual interest, by exciting prejudices against valuable public institutions, and the population of a whole town, are indications of a bad cause. Harvard, I must tell the learned attorney, is a name, that no American should ever couple with expressions of ridicule. From Harvard emanated that spirit, which, after nourishing the principles of freedom for more than a hundred years, burst into a stream that spread over the whole community. To Harvard it is we owe our Hancocks and Quincys, and Otises, and Adamses, and Cushings, and Dex-

* See Reynolds's Deposition, Appendix No. II.

ters, and Lowells, and others, the first movers and daring leaders of the revolution.

Nor is the Harvard of the present day so degenerate from its former literary and political reputation, as to merit the obloquy of the attorney. The spirit of improvement is awake there;* advances in the system of education are making, as rapidly as established habits and prejudices permit, and that the result will be highly important to this country cannot be doubted, when we see engaged in the plans of melioration, the talents and earnest efforts of such men as President Kirkland, Judges Prescott, Jackson, and Story, Mr. John Lowell, Mr. Webster, Mr. Pickering, Mr. Sullivan, Mr. Everett, and Professor Ticknor, and others, whose names might be added to the noble list.

That the Massachusetts General Hospital should have attracted the sarcasms of the attorney is not in the least surprising. Some persons might have felt disposed to exempt a public charity from such remarks; but the general tenor of the argument in this trial would entirely preclude all such expectations. We notice, therefore, without wonder, various satirical expressions besides those already quoted; as for example, "The works of surgery are full of the most tragical catastrophes, though few, perhaps, so melancholy as that which seems to have attended the outset of the Massachusetts General Hospital." And again, "The result will stand recorded as a most inauspicious augury for the Massachusetts General Hospital."

Whatever evil omens the counsellor and those with whom he sympathizes may have seen, and whatever inauspicious auguries they may have drawn, have proved as fallacious as they were uncharitable.

This institution has, since the year 1821, been gradually rising in public estimation, overpowering the prejudices of the weak, and acquiring the confidence of the intelligent, till it has now obtained a footing too firm to be shaken. Its popularity is very justly founded on the many peculiar advantages it possesses. The apartments are neat and well ventilated in summer, and comfortably warmed in winter; the food and medicine are of the best quality; all the accommodations the sick can require, are in a state of perfection not attainable in most private houses. These circumstances, together with the permitted visits of friends, the regular superintendance and interest of the trustees, and a confidence in the good intentions of the medical attendants, make this a desirable place of resort.† We find therefore, not only a general disposition in the sick poor to enter its doors, but many in easy circumstances prefer the hospital to a private house. The most distant places and States send patients to the Massachusetts General Hospital, the rich and respectable, as well as the poor and friendless; and none, besides that in which it stands, contributes a greater proportion of inmates than the State of Maine. Nor in truth has any other a stronger title to its use, for it is well known that the members of the legislature, from Maine, while that State and Massachusetts were connected, afforded an important aid towards the foundation of this hospital. The inhabitants of the seacoast of that State have learnt, that, after transporting themselves in a vessel, at a small expense, they can, if their cases are proper, enter at

* The introduction of gymnastic exercises, which I have long desired and promoted, is among the improvements now happily made; and will, in due time, give a different aspect to the sons of our Alma Mater, and to the educated youth of the whole country, spreading, as it will, by the force of so distinguished an example.

† When General Lafayette was in Boston, he visited this hospital, and was so forcibly impressed with the excellence of its arrangement, that his secretary afterwards applied to me for the details of the economy of the house, with the intention of carrying them with him to France. This hospital is also the medium through which meliorations made in other places, in the comfort and economy of the sick, as well as mechanical improvements for their benefit, are introduced in this part of the world, and soon made generally known.

a very low rate, or if they are poor, even without any fee or reward; that they can remain till they recover, if they are curable, and should they be destitute of means to return to their homes, that there are never wanting charitable individuals to afford them the necessary aid even for this purpose. They know that there is no distinction between citizen and stranger; that this hospital opens its doors to every human being; and that, whether he be an inhabitant of Boston, Eastport, or the Sandwich Islands, he gets admission on the same terms, and receives the same attentions.*

The Massachusetts General Hospital is the most noble monument of the triumph of benevolence, which Boston has to exhibit. In a community, not extensive, a subscription of more than one hundred and fifty thousand dollars, for one charitable purpose, is not an ordinary occurrence; an annual subscription of one hundred dollars each, from a considerable number of individuals, is not an event to be recorded of many towns of the size of Boston.† The benefits of this hospital have been experienced by more than twelve hundred sick persons, many of whom, no doubt, but for the assistance provided there, would have been mouldering in their graves; and it is to be considered, that this aid is not extended for a day, or a week, but that it is continued till its object is effected. The proportion of successful cases will bear a comparison with that of any known public institution. In the year in which the list of surgical cases and operations, alluded to by the Honourable Judge, was published, out of about one hundred cases of diseases and operations, only two patients died, and those from violent injuries, in consequence of which they came to the house. A success so remarkable cannot be expected to occur again, perhaps in the long annals of the institution which posterity may see. But I would not have it thought for a moment that I attribute it to any peculiar skill or attention, on the part of the attending surgeon of the institution. No, so far as obvious causes are concerned, it is to be imputed to the excellent situation and internal arrangement of the establishment; but I should think it as short-sighted, as it would be arrogant, to look anywhere for the source of this extraordinary success, but to HIM, who is the great Fountain of life and health.‡

* The public opinion of this institution is evinced by the progressive increase in the number of patients.

Remaining and received during the year			1822	122
"	"	"	1823	207
From April 1, 1824, to April 1,			1825	409
" " 1825	"		1826	525

† The president of the hospital corporation, who may be considered the father of the establishment, since by his liberality it received a donation, the magnitude of which gave an impulse to all the others, contributes two hundred dollars annually, towards its support, and to his other liberalities there is neither sum nor end.

‡ See Appendix Z.

APPENDIX.

No. I.

The following statement of the testimony of J. A. Lowell, one of the witnesses for the plaintiff, and Joseph Coffin, a witness for the defendants, is extracted from the "Report." It will show the nature and grounds of the charges against the latter. The names of the defendants are omitted, their places being supplied by letters.

Joshua A. Lowell testified, that he was clerk to the plaintiff in 1821, when he had his hip dislocated. He was called by Mr. Stearns into the room, where they were trying to set it. There were several persons present. The witness proceeded to give an account of the operation performed by Dr. I. with Dr. K. Mr. Lowell was placed across the bed. A sheet was put round the well limb, and a towel tied round the knee of the lame one. Several persons took hold of the sheet, and several hold of the towel, extending in contrary directions. The limb pointed off in an awkward position. They first extended it, and then carried it in toward the other. Dr. K. had hold of the end of it by the ancle, carrying it in: Dr. I. was feeling for the head of the thigh bone. When I first came, I found Dr. K. trying to set the limb. When Dr. I. arrived, he first examined the hip; had some conversation with Dr. K.; went out a short time with him and returned. The examination was not more than four or five minutes. Lowell inquired of Dr. I. respecting his situation. Dr. I. answered that he thought the hip bone was dislocated, and the socket a little fractured; and said, that they would set it; to this Dr. K. assented. They were not more than ten or fifteen minutes about it. They then said it was set, and well set.—Dr. I. assisted occasionally in taking hold of the end of the limb and the towel, and bearing in towards the well limb. After this they put a handkerchief round both limbs. I saw no difference in the length of the limb. Dr. K. said, Lowell must lie here three days. Dr. I. said, "Three days? you must lie there three times three; you must lie fourteen days!" Lowell asked I. if it would not be necessary for him to come over next morning. Dr. I. said no, that Dr. K. would be there, and he would give him particular charge how to proceed. Dr. K. was then in another room. When he returned, Dr. I. told him that he must bleed him next day; for he had not bled very well; and said something about medicines. Dr. K. and Dr. I. both said that Lowell was doing very well, and would not be detained from business but a short time. Dr. K. called next day and was in occasionally, and frequently at first; I was not knowing to his making any examination. He prescribed and brought liniments. Lowell was in great pain, especially the fourth or fifth day; and complained and said he was afraid his hip was not set. On the fifth or sixth day I sent over by Mr. Brooks to tell Dr. I. that my brother was in great pain, and wished him to come immediately. He came on the fifteenth day after the operation. Lowell kept confined to his bed fifteen days after the injury. He got up immediately before Dr. I. came, to have his bed made. He rested on my shoulder and the bed post while it was made. I was not present at the interview. Dr. I. sent medicine soon after his return, and at one other time. He kept his bed three days after that; making eighteen days. Ten days after this (1st of Octo-

ber) Dr. I. came the third time. He was not sent for at this time, to my knowledge. I was present. I had hired another person to take care of the store, and attended exclusively on my brother. This time Mr. Lowell stood up and rested on my shoulder, and asked Dr. I. the cause of the hollow of his hip. Dr. I. did not examine it. It was apparent outside of his trowsers. Dr. I. said it was a natural consequence, and when he gained strength it would fill up. Dr. I. said he was doing well; that his case was an important one; every thing was right then, and he would not be detained from his business more than a few weeks; but that he must be careful; it would be better that his house should burn down about his ears, than that he should make one misstep. Dr. I. told Lowell to write to him; and he would come or send medicines. He took hold of the limb and swung it, and said it was all right. I saw no comparison of the length of the limb. This lasted five or six minutes. He appeared to be in a hurry. On the 23d of October he came over a fourth time, and tarried then but a short time. When he came, he observed, that he wanted to ask my brother a few questions. My brother said he wished to ask a question. "What is the cause of the difference in length between the limbs?" I. did not make an immediate answer; he waited for some minutes, and said, it looked as though it was not set; that he was in a great hurry, and would be over again next day. His observation was, either that it looked as if it was not set, or was not in its place; and said, "To-morrow I will come and give it a thorough examination." Next time, eight or ten days after, saw Dr. I. at Eastport, about the 3d of November; I told him my brother was anxious to see him, that he was in considerable pain. I. said he was so driven that he could not possibly leave, but he said that he would come over that afternoon. I asked him his opinion; he said he was afraid the bone was not set. He said that he would come over that afternoon, unless the witches prevented. He came over on the 19th of November, with Dr. ——. I was not present.

Mr. Lowell's family was absent at this time. I attended on my brother carefully. The length of the limb and appearance continued the same; we did not discover the increased length till the 23d of October. The injured leg, very soon after the dislocation, contracted, and my brother complained of pains in his hamstrings This took place within a few days, and before he left his bed. He lay crooked up in bed with the bandage on—the limbs eight or ten inches apart—as to this I am not quite positive. The operation was performed in a small room. I saw no comparison of limbs at that time. After the operation I went to the boat. I. said K. was an old quack; that he was not fit or he would not trust him to doctor a goose, or something to that amount. I did not recollect this on the former trial, nor in giving my deposition. It arose in my mind after hearing Winslow. The witness being questioned concerning the first operation performed by Dr. I., stated that the plaintiff was laid lengthwise of the bed; a ball of cloth was placed between the thighs; the injured limb was carried out; and the ball served as a fulcrum for the leg to pry over, and be made use of as a lever. Dr. K. worked some time, and said he believed it was set, and asked me if I did not think it was not. I said I could not tell. Coffin said he thought it was not; and advised to send for Dr. I. Coffin and I went in another room, and he advised me to send for Dr. I. I asked him why he did not advise Dr. K so. We went into the room where my brother was and asked him about it. He mentioned it to Dr. K. and the Doctor consented to it.

Cross Examination.—The plaintiff lived at Lubec, near Dr. K. Dr. K. was his family physician. After Dr. I. and Dr. K. consulted, Dr. K. asked what part he should take in the operation? Dr. I. answered, "What part would you like?" Dr. K. replied, "A second-hand's birth." Several persons assisted. Dr. I. directed. Dr. K. took hold of the foot. Dr. K. cal-

led frequently; he called two or three times a day for several days. The plaintiff was moved into another house before the 23d of October. I never testified this before. He was moved by walking; and went considerable distance. My brother made no complaint of Dr. K.; but said that he had never examined. When the operation was performed several observed there was a grating. Dr. I. said that was the noise of the bone going into the socket. Dr. I. asked my brother whether it felt easier or more natural. He answered yes.

I did not hear my brother ask Dr. I. to attend further, nor request him to attend as a physician. He proposed to send a boat. Dr. I. never came with Dr. K.; always alone. Dr. K. did not make any new engagement.

I, Josiah Coffin, of Campobello, in the county of Charlotte, Province of New Brunswick, of lawful age, do testify and say, that I was at Lubec in September, 1821, near Charles Lowell, when he fell from a horse; received a bad injury of the hip by the horse falling on him. I helped to carry him into the house; was present and assisted when Dr. K. operated upon him: after which it was thought best to send for Dr. I.; he came over as soon as possible, considering the distance and badness of the ferry from Eastport to Lubec—I think in about two or three hours. After Dr. I. examined Mr. Lowell's hip, he took Dr. K. into another room; in a few minutes they returned, said Mr. Lowell's hip joint was out, and the socket that received the head of the thigh bone was fractured, and Lowell must suffer another operation—Dr. I. ordered preparations to be made, and proceeded to operate on Lowell, and I assisted as directed. Dr. K. assisted with others. Dr. I. gave directions and took his stand to manage the head of the bone. After some exertions of Dr. I. with our assistance, Lowell said that he felt the bone go in its place. Dr. I. said he felt it go into its place, and told us to give back; then asked Lowell if he did not feel more free from pain; Lowell said he did; then I. and K. examined the hip. I. took hold of the injured limb, raised it up, and turned it in every direction with ease; it appeared to move easy without giving him pain. Lowell said it felt natural; but before I. operated, the injured limb stood in an unnatural position, standing outward from the other, and could not be carried inward without giving Lowell great pain; but it appeared at this time in its proper place. I saw I. and K. take hold of his knees; but I saw no difference in the length, but both legs were of a length for any thing that I saw. Both I. and K. pronounced the bone set. I. was very minute in his directions—told Lowell that it was different from simple luxation; that the bones that formed the socket were fractured; that they must have time to unite; told Lowell that much depended on his taking good care of himself; that he feared that he would be a cripple for life—that he expected that he would be in much greater pain five or six days hence from inflammation, and that it could not be helped—that he should advise Dr. K. to make use of such means as was in reach of medical aid to keep back inflammation, and bleed him again next day; that he would send medicines over by the boat, if Dr. K. requested it. I am confident that Mr. Lowell's housekeeper was not in the room after the bed was fixed, but she handed at the door sometimes such things as were called for; but Joshua Lowell generally went for what was wanted. Mr. C. Lowell asked Dr. I. to attend him. Dr. I. said that he had a large number of sick at Eastport, that were depending on him constantly, which rendered it impossible. Lowell spoke about sending a boat for I. the next day, to which I. replied that, if his business would admit he would come; but told Lowell that he could not attend him, and desired him not to depend on him, for he did not know that he could come at

all; he did not think it necessary for him to come over, as Dr. K. was on the spot, and could come in at any time—that there was not much to be done; told Mr. Lowell he must keep still; the case ought to be left mostly to nature, as the thigh bone was in its place; and as the fractured socket was as well as the nature of the case would admit of, or words to that effect.

From twelve to eighteen days after the injury, I was present when Dr. I. examined Lowell's hip. Lowell asked him why he did not come over when he sent for him; he replied that he was very busy and had many sick to attend, but at the time you sent for me I was engaged in midwifery. Mr. Lowell told Dr. I. that he had a fit, and was afraid that he had got the bone out its place; he then got off from the bed by the help of me; then Dr. I. examined his hip, Lowell asked the reason of a hollow at the outside of his hip. Dr. I. said that the socket bone being fractured caused pain, and would for some time, but when he gathered strength the hollow would fill up, but he did not see any thing but he was as well as the nature of the case would admit of.

Question by the plaintiff. That evening or at any other time did you hear Dr. I. say, that if he had not come to me I should have been a cripple for life, and that it was a pity there was not some living spectacle of Dr. K's ignorance and quackery, or words to that effect?

Answer. I do not recollect if it was, I do not recollect it.

Question by the same. Do you know that Dr. I. demanded the payment of his bill for services in this very case at ten o'clock at night on board the packet, when he found that I was going to Boston to see Dr. Warren.

Answer. I do not.

Question by the same. When you left my house on the evening of the operation by Drs. I. and K., who remained in the room with me?

Answer. I do not know, I believe there was two or three. I believe Mr. Stearns, Mr. Sumner, and Mr. Bigelow.

Question by the same. Who was present when Dr. K. and I conversed that evening, as you have stated above?

Answer. I could not say all that was there. I believe Mr. Stearns, Mr. Sumner, and Dr. K. was there. I do not know but your brother was there.

Question by the same. Who went from my house that evening in company with you?

Answer. I think it was John Winslow.

Question by the same. Where was Dr. I. at that time?

Answer. I do not recollect whether Dr. I. was there or not, when I came away from the house.

JOSIAH COFFIN.

No. II.

Jotham G. Reynolds deposed, that he was the owner of the horse on which Lowell was riding, and was present when he received the injury. Lowell fell on his back; the horse fell obliquely across his thighs, and other parts of his body, so that his weight was more on one hip than the other.

There is a slight variation in the report of this testimony; as contained in the pamphlet of the plaintiff it is as follows:

I, Jotham G. Reynolds, of Lubec, in the county of Washington, do depose, testify, and declare—

Question by Charles Lowell, plaintiff. Were you the owner of the horse by which I got hurt, in 1821?

Answer. I was.

Question by the same. Were you present when I received the injury?

Answer. I was.

Question by the same. Did I fall on my back, or side?

Answer. It appeared to me, you fell on your back.

Question by the same. As I fell, did the horse fall directly across my thighs?

Answer. It appeared to me, at the time, that the horse fell obliquely across the thighs and other parts of the body, *and it appeared to me, that the horse fell partly betwixt the legs*, so that his weight was more on one hip than on the other.

No. III.

The following extract from the Address of the Attorney for the Defendant, will give an idea of its style and manner.

If any thing could add to our perplexity on this occasion, it would be to consider the determination with which these learned gentlemen undertook to put this poor plaintiff so resolutely to the rack, upon the strength of this precipitate and ill advised opinion. It is true, Dr. Townsend says, they thought there was no great chance for him. And Dr. Mann declares it to have been their unanimous opinion, that the attempt would prove unavailing. It was on this groundless supposition, and when there was so little prospect of success, according to their own avowal, that they undertook to put it to the test. What with their warm baths and smart cathartics, profuse bleeding, nauseating doses and preparatory drenches to relax the muscles, and then sheets and cords and bandages and pullies, with the whole accumulated force of the mechanical powers employed to wrench his limbs, according to Desault's most approved method of reducing luxations, until the head of the bone was fairly lifted from the socket it was thought to have formed—the agonies endured by Lowell under this operation could scarcely have been less than those of Damiens, for his attempt to assassinate Louis the XVth; for there is a point beyond which human anguish cannot extend, and at which a merciful insensibility commences. An account of the execution of Damiens, found in the French Philosophical Dictionary, is as follows: He arrived at the Place de Greve at a quarter past three, regarding with a dry and steady eye the spot and instruments assigned for his execution. After some painful preliminaries, as well to be omitted, they proceeded to the business of dismemberment. Four powerful young horses, which had been provided, made incessant efforts for the space of fifty minutes to tear his limbs from his body, without being able to effect the object. At the end of this period, he being still alive, they were obliged to make use of their instruments to separate the joints, as had also been done in 1610 in the case of Ravaillac. He breathed a few minutes after the thighs were removed; and it was not until his arms were amputated, that he gave up the ghost. This operation lasted, from the time he was placed upon the scaffold until he ceased to palpitate, nearly an hour and a half.

How long the present experiment was continued, or how often it was repeated, or the patient fainted under the operation, does not appear from the depositions. His own courage, it seems, was unexhausted. Why, then, with all this prodigious power employed, why was not the dislocation reduced? For this plain reason; because no dislocation existed. Simply, because the thing was impossible: there was nothing to reduce. The wonder is not that the operation was not successful, the only wonder is that the

plaintiff survived it. If the plaintiff had suffered any real injury, at the time he brought this action, from ignorance or unskilfulness, to whose hands is it to be attributed? By their own statement these deponents persevered in performing the operation, which Dr. I. had declined to undertake, until it was proved to be perfectly impracticable. The presumption, therefore, is at least equal against any dislocation: the burden would certainly be upon them to prove that one existed; and if the plaintiff had been advised to prosecute this formidable phalanx, for thus violently undertaking to set a limb, that did not prove to be out of its place,—upon this authority, upon this reason, and on this evidence, what could have been their defence—and what could have protected them against a verdict, excepting the charitable supposition, that they must have been under some strange planetary influence or infatuation?

No. IV.

Interrogatories proposed to John C. Warren, James Mann, Thomas Welsh, David Townsend, and Robert Hewes, by the plaintiff:

Question 1st. Did you make an attempt last December to set or replace the head of my thigh bone, which had been displaced from its socket; or were you in consultation on my situation, previous to any operation in your presence on me; or were you present, or did you take any part in an operation for the above purpose?

Question 2d. Who were the persons with whom you consulted?

Question 3d. What was their opinion of my then situation and real injury; and were they unanimous in that opinion?

Question 4th. Did their opinion coincide with yours; and what was yours, and is your opinion of my case?

Question 5th. If a surgeon should undertake to set a dislocated limb of this kind, and should use no greater force than could be applied with the naked hands, without any thing fastened about the parts, to enable them to hold on, &c.; and should not succeed at all, would you say that he used the means, or acted with the skill and management, that a surgeon ought?

Question 6th. Before a surgeon should give up in such a case, or say that the limb was set, when it was not, ought he not to bleed the patient, or use other means of relaxation, and then make another effort?

Question 7th. If the limb in the case like the above, hung off from the body in a very awkward and unnatural position, and could not be moved in towards the other limb, without appearing to give extreme pain, ought not a surgeon of common or ordinary skill, to know from that circumstance, as well as from the circumstance of the injured limb being three inches longer than the other, that it could not be in its proper place?

Question 8th. Is not the return of the thigh bone to its original socket usually accompanied with so loud a noise, that it must be heard by all in the room, and could not be mistaken, especially by a prudent and discerning surgeon?

Question 9th. Would not a common caution and attention require the attending surgeon to examine the limb occasionally; especially if the patient should complain of much pain, long after the injury?

Question 10th. Would not a surgeon of common and ordinary skill and care have compared the length of the injured limb with that of the other?

Question 11th. Is it not a general rule, that an injured limb like the above named, being several inches longer than the other, is an indication that such limb is not in its proper place?

Question 12th. In attempts to set and reduce a dislocated limb of this kind, is not a fulcrum, or something to answer its purpose, necessary?

Question 13th. If a person, whose hip or thigh bone was dislocated, as mine is, and set in three hours after the injury, should lie on his back fourteen days without turning, having his knees tied together, would it be in his individual power to get the bone out of the socket again, while in that situation; or to make the necessary extension of the limb to lodge the head of the thigh bone three inches below the socket, where mine is?

Question 14th. Is not the natural action and re-action of the muscles and cords about the hip, such as to require something of a relaxing nature, and a powerful force to be applied in order to extend the limb three inches, or even one inch, beyond its natural length?

Question 15th. If it were possible for one in the situation described in the thirteenth question, to get the bone out of the socket again, would not those strong cords and muscles immediately contract and draw the limb up, instead of causing such an extraordinary extension as to make it three inches longer?

Question 16th. Is my case any thing more than a simple luxation?

Question 17th. From your examination of my case, do you think that the head of the thigh bone of my injured hip or thigh is lodged in the ischiatic notch?

Question 18th. Would it not be necessary to extend my leg considerably in length, before the head of my thigh bone, last named, could be removed from its present lodgment?

Question 19th. In December, 1821, did you make an attempt to set or replace the head of my thigh bone, which had been dislocated from its socket by a downward luxation?

Question 20th. Did you then, or do you now, consider my case any other than a simple luxation?

Question 21st. When the hip is disjointed, is not the difference in the length of the injured limb and the well one so great as to be a visible and decisive proof of dislocation existing?

Question 22. If two surgeons were called in to see a person whose hip had been disjointed only two or three hours before, and they should operate—say that they had set the bone, and should subsequently attend the patient for the space of six or seven weeks, and in that time the patient should repeatedly tell them that he was in great pain, and that he feared something was wrong; and they should at their several visits say that all was right, and that he was doing well,—but it should eventually prove that the bone was all the while out of joint, would you not say, that it was a strong mark of gross ignorance, or inattention on the part of the surgeons, that they had not discovered the true situation of the limb before?

Cross interrogatories put to John C. Warren, M. D. and the other deponents on the part of the Defendants.

Interrogatory first. What is your profession or business, and how long have you been engaged in it?

Second. Have you ever reduced a luxated hip joint? and how many? did you ever see a downward and inward luxation of the hip joint? did you reduce it? did you ever know any one reduce such a luxation?

Third. Do you know Charles Lowell, of Lubec? if you do, please state how, and when you became acquainted with him?

Fourth. Was Lowell's hip joint dislocated when you saw him—if so, what were the reasons that induced you to think so? Please state particularly and minutely all the facts and appearances respecting it?

Fifth. Do you not think it possible you may have been in an error in your opinion in the case?

Sixth. Have you never before in the course of your professional prac-

tice made as great a mistake as it would be to pronounce Lowell's hip joint dislocated when it was not?

Seventh. Do you not consider Lowell's case of such a nature as that medical men of high standing in the profession would be likely to differ in opinion respecting its present situation, or at the time you saw him; that is, whether it then was or now is dislocated or not?

Eighth. Would not a luxation of the joint, or fracture of the lower edge of the acetabulum, the necessary violence done to the parts in producing these and replacing the bone, together with a consequent rheumatic affection of the limb, hip, and pelvis, attended with some distortion, particularly of the latter, be alone sufficient to account for all the appearances in Lowell's case, when you saw him, without supposing the head of the bone out of its proper socket?

Ninth May not the soft and bony parts about the hip joint, especially in a muscular man, be so injured as to render it impossible for the most competent surgeon, some months after the injury, to judge what was the actual situation of the patient, or what ought to have been done for him at the time of the injury?

Tenth. Did you make any attempt to relieve Lowell's disability? if so, what were the means made use of, and what was the result? Please to be minute and particular.

Eleventh. Do you consider dislocation pullies necessary in reducing luxations of the hip joint? what proportion of cases can be, or are reduced without them? do not medical men differ in opinion with regard to their being used at all? and have you never known or heard of a case or cases being successfully treated by the hand after the pullies had failed?

Twelfth. Have you never known a case of dislocation where the surgeon first called, declared the pullies necessary to reduce it, and while preparations were making therefor, another man stepped in and reduced it by hand? and who was the surgeon first called in the case?

Thirteenth. Do you think any blame should attach to a surgeon for not using the pullies, when he succeeded perfectly well in reducing the dislocation without them?

Fourteenth. If you are of opinion that Lowell's hip joint is now out of place, do you not deem it possible that it might have been reduced at the time of the original injury, and afterwards displaced by accident or misconduct of the patient, and this done without the knowledge of the surgeon, and without his being able to prove the fact in a Court of Justice?

Fifteenth. Is it customary for a surgeon after once reducing a luxation, to watch the patient night and day to prevent such accident or misconduct, or to take with him in his visits a credible witness to prove the correctness of his own conduct, and guard himself against the malice and intrigue of a litigious patient?

Sixteenth and *Seventeenth* Interrogatories in the original, *crossed out.*

Eighteenth. What weight would you give to the opinions of common people standing by or assisting to reduce a dislocated hip, as to the professional skill with which the operation was performed, or are people in general competent to describe the means used by a surgeon to reduce a dislocated hip?

Nineteenth. What is Robert Hewes's character as a surgeon?

Twentieth. Do you know any other matter or thing advantageous to the Defendants, or either of them? if so, please to state the same as particularly and minutely, as if thereto specially interrogated.

John C. Warren, of said Boston, Doctor of Medicine, to the said several interrogatories, and cross interrogatories, doth testify, depose, and say:

1. To the first he saith, that in December last, he did make an attempt to replace the thigh bone of Charles Lowell, the person here present; this was done at the Massachusetts General Hospital. I did enter into consultation with several gentlemen upon his situation, previous to any attempt

being made to replace the dislocated bone, and took a principal part in that operation.

2. To the second he saith, the persons with whom he consulted were Doctors Townsend, Welsh, Mann, and Spooner, consulting physicians of the Hospital.

3. To the third he saith, that they were unanimously of opinion that the hip was dislocated.

4. To the fourth he saith, that their opinion did coincide with that of this deponent. His opinion then was, and still is, that the hip was dislocated.

5. To the fifth he saith, that it is difficult to give a precise answer to this question, on account of the generality of its terms. This deponent has heard of cases in which manual force only has been applied with success. But he is of opinion that in case a surgeon should use manual force only, and know that it did not succeed, such surgeon could not be said to have acted with proper skill and management, and used due means to reduce the dislocation, if he applied no further force than could be applied by the naked hands.

6. To the sixth he saith, certainly he should.

7. To the seventh he saith, the circumstances enumerated in the question, would lead this deponent strongly to suspect that the limb was out of its place; but he should not consider these indications decisive.

8. To the eighth he saith, it frequently happens that such a noise is heard, but not uniformly, when the bone is restored to its socket.

9. To the ninth he saith, that if the patient should remain apparently without much pain, he should not consider such examination necessary; but if the patient should complain of much pain, and for a considerable time after the injury, he should think such an examination ought to be made.

10. To the tenth he saith, Yes.

11. To the eleventh he saith, that the circumstance of the limb being three inches longer than the other, is an indication that the limb is disordered, but is not a necessary indication of a dislocation of the bone. It might proceed from two other causes; either from a fracture of the neck of the bone, with a relaxation of the muscles, or from a simple relaxation of the muscles.

12. To the twelfth he saith, that it is generally necessary, but is not in all cases indispensable.

13. To the thirteenth he saith, that in his opinion it would not be in the power of the patient to displace the bone, under the circumstances stated.

14. To the fourteenth he saith, that the structure of the part is such as to require great force to extend the limb one inch beyond its natural length.

15. To the fifteenth he saith, that it would depend upon the direction in which the head of the bone should be forced out of its socket. If forced downward, the limb would be extended; if upward, it would be shortened.

16. To the sixteenth he saith, that he has no reason to think that it is.

17. To the seventeenth he saith, that that is his opinion.

18. To the eighteenth he saith, No.

1. To the first cross interrogatory, put on the part of the Defendants, he saith, that his profession and business is that of a physician and surgeon, in which he has been constantly engaged for the last twenty years.

2. To the second he saith, that he has frequently reduced a luxated hip joint; the number of instances he cannot recolleet. He does not recollect ever to have seen a downward and inward luxation of the hip joint.

3. To the third he saith, that he saw Mr. Lowell for the first time some time during the last winter; the precise time he cannot tell. The place was Clark's Tavern, in this place. He was removed in the course of a few days to the Hospital, during which time the deponent saw him several times.

4. To the fourth he saith, that in his opinion, the limb was dislocated at that time, and his reasons for believing so, were, first, that the knee hung out from the other in an awkward and unnatural manner; secondly, that the thigh of the injured side was longer than the other, or in other words, that the knee projected lower than the other; thirdly, that the flexor or hamstring muscles were contracted, so as to keep the leg continually bent; fourthly, that the trochanter major was not to be felt in its proper place; fifthly, that the head of the dislocated bone could be felt in an unnatural position, in or about the ischiatic notch; sixthly, that the patient had not a free and natural use of the limb, but its motions were constrained in such a manner as happens only in the case of a dislocated limb, by the head of the bone being lodged in the ischiatic notch; that is, in a dislocation backward and downward. The dislocation of Mr. Lowell, in the opinion of this deponent, was one of that character.

5. To the fifth he saith, he doth not.

6. To the sixth he saith, that he doth not recollect any.

7. To the seventh he saith, that it was a dislocation difficult to discover; but one about which, in his opinion, men of high standing in the profession could not differ.

8. To the eighth he saith, that he thinks the appearances in Mr. Lowell's case could not have been produced by any or all of the circumstances enumerated in this question.

9. To the ninth he saith, yes, such a case may exist.

10. To the tenth he saith, that, as before stated, he did make such an attempt; the means were these: the patient was placed upon his right side and secured to a table, and further secured to a neighbouring wall by a sheet passed between the thighs, and a force was applied immediately above the knee of the injured limb, in a direction to draw it forward and inward. At the same time a force was applied at about the middle of the thigh, at right angles with the limb, in such a direction as to draw the head of the bone toward the socket. The forces were gradually and alternately increased for the space of about an hour, and till all prospect of success was at an end. The force at right angles was applied by pullies, and the other by the strength of several persons by means of bandages and cords. By way of preparatory measures, the patient took a powerful cathartic in the morning, and went into a warm bath. And in order to relax the muscular powers more fully, immediately before the operation he took nauseating doses of tartrate of antimony, and was bled as freely as possible.

11. To the eleventh he saith, that he doth consider them necessary, though not indispensably so; the greater proportion of cases can be, and are reduced without them. There is some difference of opinion with respect to their use, though the most celebrated authors decidedly recommend their use. He has never known a case treated successfully by the hand after the use of pullies had failed.

12. To the twelfth he saith, he hath never known such a case.

13. To the thirteenth he saith, that he should not.

14. To the fourteenth he saith, that if the dislocation had been reduced, he should think it possible the bone might be thrown out of its place again, by the application of considerable force, or the use of considerable motion of the limb, soon after the reduction.

15. To the fifteenth (objected to with the three following as above) he saith, No.

16. To the sixteenth he saith, that in his opinion, a surgeon, who employs the best means in his power, ought not to be responsible in damages; and that a surgeon who has opportunity to examine a case of dislocation, immediately after the injury, other circumstances being equal, has better means of judging of the nature of the case, than one who examines the case several months afterwards. Still he is of opinion that the case may be

such, that it may be quite apparent, several months afterwards, what the real nature of the injury was.

17. To the seventeenth he saith, that not having had a distinct account from Dr. I. himself, of the mode of treatment practised in Mr. Lowell's case, he does not feel qualified to give an opinion on his practice.

18. To the eighteenth he saith, that he should attach no importance to the opinions of persons thus situated, as to the professional skill with which an operation was performed; but he thinks that intelligent persons without professional skill, might describe with sufficient accuracy the visible means used in the operation.

19. To the nineteenth he saith, that the said Robert Hewes has some reputation for reducing dislocated limbs.

20. To the twentieth he saith, that he knows nothing more, unless it be material that the letter hereto annexed, dated April 12, 1822, marked A, was written by this deponent at the time it bears date, and forwarded according to its direction. This letter is hereto annexed (the said Lowell objecting thereto) at the request of A. Peabody, Esq. representing Dr. I. At the time of writing that letter, as far as this deponent can recollect, he was not aware that any judicial proceeding was pending on the subject.

JOHN C. WARREN.

Note.—On reviewing my deposition, I find myself not altogether satisfied with the degree of precision in the expressions of some parts of it; and endeavouring to recall to memory the reasons that might have prevented me from giving it that attention, which, since the formidable criticism to which it has been exposed, might be thought to have been required, they seem to be nearly these—When I was called on to give this deposition, I did it under the impression that there would not be any important question as to the anatomy or surgery of the case. It would turn, as I thought, and still think it should, exclusivey *on the charge of negligence.* My deposition I regarded, therefore, as *a mere matter of form.* No part of the deposition was written by me, but by the commissioner; nor did I myself read it over after it was written. Finally, the period of examination was more than two years from the accident; and I was called suddenly to answer questions on points, most of which had not entered my mind from the time I saw the patient till that moment. These facts will, I hope, with every candid mind, form my excuse, if any be required, for a want of exactness in the language of the deposition. For illustration, I will furnish an instance of the description alluded to. Being asked, whether I had ever reduced a dislocation of the hip joint, answered, that I had reduced a number, but could not recollect *how many.* The defendant's attorney, adverting, in his manner, to this answer, said, that I "was not able to charge my mind with the number of cases," or words to that effect; obviously meaning to intimate, that the number of hip dislocations I had reduced, was not likely to be worthy of consideration. Now any reflecting person will perceive at once, the impossibility of my having been able to answer suddenly, and without previous consideration, what number of cases of this or any other dislocation I had in the course of practice reduced. Since his publication I have been necessarily led to ascertain, as nearly as possible, what number of cases of hip dislocation I have seen; though I would by no means aver I have recollected the whole number.

No. V.

LETTER TO DR. I.

Boston, April 12, 1822.

Dear Sir,

In reply to your letter requesting my opinion on the question, whether Mr. Lowell's dislocated thigh had been reduced, I beg leave to say:

First—That every person will understand it to be impossible for me to say positively, that the limb was not reduced, provided especially there is any positive testimony that it was so.

Second—That my opinion certainly was, that the limb had not been reduced, for the following reasons.

1st—That if it had been reduced, it would not have been again luxated without great violence, on account of the depth of the socket; and I did not understand that any such violence was inflicted after the efforts to reduce it.

2d—That this dislocation is so difficult to reduce, that I presumed it would remain unreduced often in the hands of the best surgeons, and with the best means. If such a case were to come to me, I should have many doubts of being able to reduce it in a muscular man, even in its most recent state, especially without dislocation pullies.

For these reasons my opinion was, that the dislocation which was thought to be reduced, was not so, in fact. I would not, however, oppose an *opinion* to any positive testimony.

In any case, I presume that no blame is to be attached to you, as the case is a very difficult one, the worst of four kinds of dislocations of the hip, and I presume, also, that you did every thing that circumstances admitted.

I am, Sir, your very obed. servt.

J. C. WARREN.

It may be a question, whether the acetabulum had been fractured; but as time enough for it to unite had elapsed when I saw him, I can give no opinion on this point.

Deposition A.

I, ——— A———, testify and say, that in the month of June, in the year 1822, I examined Charles Lowell, then at Eastport, respecting an injury of his hip, which he stated to have happened the fall before. My examination was lengthy and critical, and my opinion then was, that the thigh bone was not out of joint; and I have not altered my opinion since. From the nature of the injury as described to me by the said Lowell, it could hardly be possible that the hip should be dislocated. A fall on the hip, with the weight of a horse upon it, would be likely to break the bones of the pelvis, and might drive the head of the bone through the bottom of the socket, but could not dislocate the joint; and, in my opinion, if there is any derangement of the bones, it is a fracture, and not a dislocation. In that case, it would not have been in the power of Dr. I. or any other medical man to have rendered the said Lowell any effectual assistance, more than to have administered remedies to keep down inflammation; they could not have altered the situation of the bones. As for the apparent lengthening of the affected limb, I think that is owing to the preternatural contraction and relaxation of the muscles situated about the hips; and is made to appear so by the twisting of the bones of the pelvis on the spine. Any person, when sitting in a chair, can, by an exertion of the muscles, make one knee project beyond the other, as much as Lowell's did when I saw him. The same lengthening of the limb takes place in a disease of the hip, called the hip disease, which partakes of the nature of white swelling, where no external violence has been received. It is difficult to determine in case of injuries of the hip precisely what the injury of the bones is; but it has frequently happened, within my knowledge, that by a fall directly on the hip joint, though the bone was not dislocated, as was evident by the natural position of the foot and limb generally, and from its being moved by the hand of the surgeon in all directions, yet the patient has never recovered from his lameness; and in several instances they have never been able to walk afterwards. In cases where the thigh bone is dislocated backwards, and the head of the thigh bone rests on the back part of the broad hip bone, the limb will be a little shortened, and the foot will point towards

the other foot, and cannot be turned outward in the least. In case the head of the thigh bone should be lodged in the ischiatic notch, so called, the limb would or might be a little lengthened; but the foot would be turned pointing to the other foot, and could not be turned outward in the least. Both when the head of the bone is on the back of the hip bone, and when in the ischiatic notch, the head of the bone can be distinctly felt by the hand. When the head of the thigh bone is dislocated downwards, and rests in the thyroid hole, so called, the trochanter will be misplaced, and the head of the bone will be felt on the side of the perineum, between the scrotum and anus, and the foot will be turned out. Very great violence done to the parts, and consequent swelling, might render it difficult to ascertain, by feeling, the position of the head of the bone soon after the injury; but when the swelling had subsided, it might be ascertained by the touch. As to the length of time, which may elapse after a bone is dislocated, before it will be impossible to reduce it, it is uncertain, and probably may differ in different cases. But the time that a joint may remain dislocated, and yet admit of being replaced, is longer than has been generally supposed. I reduced a dislocated shoulder that had been out seven weeks, another that had been out nine weeks, and one that had been out nearly four months. I should not think that a hip joint, having been out of place six or even eight weeks, would render it impossible to reduce it. It might even be a more favourable time for the operation, than immediately after the accident, especially if the soft parts at first were much bruised and swollen.

I do not think that the mechanical powers, such as the wheel and axle, or the pullies, are necessary to reduce a dislocated hip, or any other dislocation. They have sometimes been used with effect, but they have oftener been injurious; and what can be effected with them, can be effected without them. It is not the quantum of force which reduces dislocated bones, so much as it is the direction of the force; and this can be given by the hand of skill, better than by pullies, &c. In reducing the hip joint it cannot be done by direct pulling; but we take advantage of the thigh bone as a lever to move the head of the bone from the place where it may be lodged, and bring it into its former situation. In some cases the fulcrum is some of the bones of the pelvis; in others we have to supply it by some external body.

Question by Defendant's attorney. Did you ever reduce a dislocated hip? And if so, please to state the manner.

Answer. I once reduced a dislocated hip joint. It was dislocated upward and backward; and after pulling it in every direction but the right, it was reduced easily by carrying the knee towards the patient's face. I had the assistance of two men only.

Question by the same. Would the distortion of the pelvis, by contraction of the muscles, produce an apparent lowering of the hip joint, or a hollow up the hip?

Answer. It might, and probably would.

Question by the same. If the head of the thigh bone were forced through the bones of the pelvis, would that produce in any measure the same effect?

Answer. It would.

Question by the same. Is the dislocation of the hip joint an unusal occurrence? and might a skilful surgeon fail in any attempt to reduce it?

Answer. A dislocation of the hip is very rare; and probably not one medical man in ten would be able to reduce it.

Question by the same. Would a failure to reduce a dislocated hip subject a man to the just imputation of ignorance in his profession?

Answer. I should think not, for men of science, and reputed skill, have failed.

Question by the same. Do you know Dr. I. of Eastport? And if so, what do you think of him as a man acquainted with his profession?

Answer. I have been acquainted with Dr. I., and think him above mediocrity in the knowledge of his profession, especially in anatomy.

Question by the same. May not physicians and surgeons disagree in opinion respecting a disease of the hip, without the imputation of ignorance or negligence?

Answer. Men of science and skill have often disagreed in such cases.

Question by the same. Were the head of the thigh bone lodged in the ischiatic notch in Lowell's case, how would the limb act? Would it be as when you saw it at Eastport?

Answer. When the head of the bone is lodged in the ischiatic notch, the foot would be turned inward, which was not the case with Lowell when I saw him.

Question by the same. What is the situation of the ischiatic notch in the living subject? And is it filled or partially so, with any substance?

Answer. In the living subject the ischiatic notch is filled with a firm strong ligament, which is again covered with muscles, so that the head of the bone could not sink much into it.

Question by plaintiff's counsel. When you were at Eastport, before you examined Charles Lowell, and while you were at some distance from him, did you say to any one, that Lowell's hip was not dislocated, or to that purport, and if so, to whom did you make the observation?

Answer. I do not recollect that I did, and am very confident that I did not.

Question by the same. Did you tell Lowell he had better drop his action and try to get well, which would be better than to try to get damages of the Doctors?

Answer. I think I did.

Question by the same. How long did you take to examine Lowell's hip joint, and did you attempt in any manner to restore it to its proper place and appearance?

Answer. I did not measure the time, but put him in various positions, and examined him in company with Dr. B. till I was satisfied it was not out of joint. I did not make any attempt to replace the bone.

Question by the same. What did you prescribe for the remedy of his limb, and what encouragement did you give him?

Answer. I believe I advised him to make an issue on his hip, and keep it open a long time.

Question by the same. Did you tell him he would probably be a well man in a year, if he followed your prescription, or to that effect, and did you give it to him in writing?

Answer. I think it probable that I gave him encouragement that he might get well, or better than he was then; but do not recollect whether I gave him a written prescription or not.

Question by the same. Had you heard Dr. I.'s representation of Lowell's case before you saw Lowell?

Answer. I had.

Question by the same. If there was a distortion of the pelvis so as to occasion the appearance of Lowell's limb, would it not have occasioned pain at or near the back bone?

Answer. I do not know that it would. In cases of disease of the hip joint, where the pelvis is distorted, the patient does not complain of pain in the back to my recollection.

Question by the same. If there were a dislocation of the head of the bone into what is called the ischiatic notch, would it not occasion the same appearance that Lowell's exhibited?

Answer. I think not.

Question by the same. How do you account for the hollow appearance in Lowell's hip, at the place where the head of the thigh bone was inserted, and did you feel it to be hollow when you examined it?

Answer. I did not perceive any more hollow on the hip joint, than might be accounted for from the effect of the muscles, or a fracture of the pelvis.

Deposition B.

I, —— B——, of St. Andrews, in the Province of New-Brunswick, Physician of lawful age, on oath, do testify and say, that I was this day (June 13th, 1822) present at an examination of Charles Lowell of Lubec, for a disease or affection of the left hip joint, and am of opinion that it does not arise from dislocation at present existing, but from affection of the muscles, or some other cause.

Question by Plaintiff. Did you ever reduce and set a thigh bone which had been dislocated from its socket by a downward luxation?

Answer. I never did.

Deposition C.

Dr. C. was introduced as a witness by the defendants, and was requested to explain to the jury the structure of the different parts of the subject, and to exhibit the varieties of dislocation by the bones of the skeleton (viz. the pelvis and thigh bone) which were produced for that purpose. This mode of proceeding was objected to by the plaintiff's counsel; who proposed, in that case, to offer the plaintiff himself to the personal inspection of the jury. No opposition being made to the course by the defendant's counsel, the plaintiff was submitted to the examination of several of the jurors. Dr. C. exhibited the manner in which several dislocations took place; viz. two forward, one of which was upward and one downward, and two backward, both upwards. He indicated the position of the ischiatic notch. In dislocation into that notch, he testified, that the knee and toe turn in. It was impossible for the knee to turn outwards in a dislocation into the ischiatic notch. The plaintiff's knee and foot are canted a little outward. Sir Astley Cooper was considered the greatest authority in surgery. The witness thought the plaintiff's injury was a fracture and derangement of the pelvis. The ischium might have been broken, and some tuberosity forced and felt about the notch. From the nature of this testimony, it is not capable of being perfectly reported. He testified to the respectable standing of Dr. I. in his profession.

Deposition D.

Dr. D. concurred in the opinion of Dr. C. and confirmed the general points of his testimony. It was an injury to the bones of the pelvis. A surgeon could be of no benefit to Lowell. He testified also to the respectability of Dr. I.

Appendix E.

Concluding Address of the Attorney for the Defendant.

Let not a judgment against the defendant be thought light. The end of this action is to annihilate the character and usefulness of the defendant; and such, undoubtedly, Dr. I. feels the attack with a sensibility proportionate to

the injury aimed. His standing and reputation, therefore, form a trust, which he feels it a duty to defend for the benefit of the rising and thriving community, which comprises his present sphere of practice; and also as the principal means he has, in connexion with the little prospect of fortune it affords, under Providence, of executing the humble scheme of happiness and improvement he has in contemplation for himself, for his family, and society. Of all this the verdict outrageously sought by the plaintiff, would simply rob him for ever. The consequences extend therefore to the community, which is hardly less interested in the result; and what is the consequence of a limb like Lowell's, supposing he should never perfectly recover it, compared with the usefulness of such a physician as Dr. I. entirely lost to the present scene of his practice? Is there a member of the profession in this section of the country, that any individual who has witnessed the full exposition of this case, in case of any accident happening to himself upon this spot, requiring the aid of a surgeon, would select in preference to this defendant? And is there any portion of the state, that would not rejoice to receive the benefit of his skill, if it should be expelled from the county of Washington? Rise, then, above the influence of prejudice, and restore him to a society which is capable of appreciating his expertness and fidelity; and inspire him with a due confidence in the justice of his fellow citizens to protect him against such an unprovoked and unfounded assault upon his rights, his feelings, and his practice. Sustain him against this fiery trial of his patience, and bring him out of the furnace without allowing a hair of his head to be harmed. A verdict against him would be his ruin. Call a soldier a coward, a clergyman a hypocrite—and judge what it is for a physician to be branded as a quack. Compared with his character, a treasure so deservedly endeared to him by the unrighteous manner in which it has been assailed, he values not his property a feather. If the jury arrive at the question of damages, he does not request them to trouble themselves about fractions. He does not entreat your mercy, he makes no claim for commiseration; and will not thank you for forbearance. By his express instructions his counsel are not only warranted, but required to urge upon you not to compromise his character by a verdict for mere nominal damages. If you are obliged by your oaths to find a verdict for the plaintiff, he does not ask of you to stint them. Give him the whole length of the lash.—Give to the plaintiff, in that case, his just measure, heaping full, and running over. Take from the defendant, if you please, the pittance of his humane and patient industry; and take with it all further incentive to emulation and exertion; blast that reputation, which is the fair growth of a brief and honourable practice; steep him in poverty to the very lips; let him exhaust the cup you shall mingle for him to the very dregs; bring down upon him the whole weight of the Harvard Medical Faculty, piled upon the Massachusetts General Hospital; let the Chaldæans come upon him, and the Sabæans make three bands; and the wind from the wilderness smite upon the four corners of his house, until he shall be reduced to sit down among the ashes and scrape himself with a potsherd—nothing can deprive him of the consciousness of integrity, derived from the consolation of having performed his duty.

Appendix F.

The feelings experienced by the unfortunate Plaintiff, and the views he entertained, are expressed in the following conclusion of his Pamphlet.

The amount of this interesting subject, appears to be this: viz. That I, in the prime of life, with a simple dislocation of the hip, fell into the hands of Doctor I. and Dr. K. by whose ignorance and negligence I have lost the use of my hip for life; that after suffering five excruciating operations, and expending about 1000 dollars in doctors' bills, nurses' bills, &c., and with a total loss of two years' time, and 1000 dollars more in an appeal to my coun-

try for redress, I have lost a just and plain cause, and that too, by the improper management of the Court, and the corrupt influence of a combination of unprincipled men, from various parts of the county of Washington.

I have now told my plain and unvarnished tale of woe, and when my readers arrive at this point, I trust they will be prepared to say, that I have unfolded to them a scene of injustice and oppression, which has few examples in the history of this country, and such as ought not to be passed over in silence by the government and people of this independent state This cause is of no common interest to society, or the parties engaged; for every good citizen must feel a deep interest in arresting the progress of imposture and quackery. It is one of peculiar bearing on the vital interests of many, for it strikes at the very root of *fraud and deception* in every profession; and although I am a ruined man, and life has few charms remaining for me; although I have been extremely unfortunate in the expected result of this suit; there are no two acts of my life, on which I can reflect with more heartfelt approbation than the prosecution of I. and K., and the publication of this pamphlet; and through subsequent life, I shall look back with the proud satisfaction of having been a public benefactor, in exposing these Doctors and the Judge's charge to the world.

Although I am satisfied, that this work is founded on the immutable basis of truth and justice; and that it is couched in decorous and appropriate, though strong terms; yet I have no doubt it will be assailed by some one of the numerous host of quacks, pettyfoggers, and gamblers, with whose base influence I have had to contend during this protracted litigation; but unless they do it in an ingenuous manner, and in their own proper names, I shall treat them with that silent contempt which such communications deserve.

* * * * * * * * *

Although the loss of my property, and the treatment I have received, the loss of the use of my hip, and the pains and sufferings which I am doomed to endure through the round of time; the recollection of past enjoyments, together with other misfortunes (not interesting to the public) do at times all rush upon my mind with a weight and impetuosity, beyond the power of human efforts alone to sustain. Yet, amidst all these calamities; amidst the clouds of darkness and doubt, injustice and oppression, I can look forward and see the glimmerings of the sun, which seem to indicate the approach of better days. There I can behold the most profound and consummate wisdom, guiding the destinies of men; and I feel a consciousness, that the Divine blessing on the purity of intention, and the energy and perseverance which have characterised my hitherto rapid, but unsuccessful career in life, will yet support and conduct me through the labyrinth of life's thorny maze, to the peaceful shores of eternal blessedness, "where the wicked cease from troubling, and where the weary are for ever at rest."

CHARLES LOWELL.*

Machias, January 1, 1825.

Appendix Z.

Some of those who are not fully acquainted with the nature and extent of the attacks made on me, will, I am aware, be disposed to question the expediency of bestowing the attention to the subject, I have done; and per-

* Since the greater part of these sheets were printed, I have seen Mr. Lowell, the plaintiff, for the first time, since I gave the deposition. His anticipations of evil had, I found, been realised to their full extent. His health had been gradually impaired by the change of habits produced by his lameness, and his spirits broken by repeated misfortune.

To those who feel an interest, as I have no doubt many persons will, for this unfortunate man, it may seem that I ought to have defended his cause, and shown the real character of the treatment he had received. This, however, could not be done without exhibiting the conduct of others in an unfavourable light; and I have been desirous to avoid this, any further than was called for by self-defence.

haps they may also justly doubt the propriety of involving, in this discussion the opinions of respectable gentlemen, in and out of the medical profession, though I have omitted their names; and notwithstanding the reasons assigned in the beginning of this pamphlet.

For the satisfaction of those who may not have seen, in the attacks of the defendant's attorney, sufficient reason to justify the course I have taken, I will make an extract or two from a series of publications which appeared in newspapers printed at Eastport, the town where the defendant lives. Whether they were written in any part by the defendant, I know not. They are published in the place where he resides, and it is obvious that some of the facts must have been derived from himself.

This series is begun, by republishing a review from the Boston Medical Intelligencer, which these writers think proper to attribute to me or my friends, because it contains some complimentary remarks, although it has also opinions diametrically opposite to those expressed by me, and in every feature bears a character which distinctly marks it as not emanating from me. Of the succeeding numbers, the following may serve as specimens.

Extracts from the Eastport Centinel of December 10, 1825*.

"Mr. Folsom,

"A correspondent of your paper, a short time since, made some remarks on an article in the "Boston Medical Intelligencer," purporting to be a Review of the Trial of Lowell vs. Doctors I. and K. which has made no little stir in the medical world, and, before it is suffered to remain in quiet, I am rather apprehensive will make still more.

"By publishing the following remarks, with the extract from the "Review," you will confer on me and others a particular favour.

"Dr. I. will not contend with Dr. Warren that it is impossible the head of Lowell's os femoris is in the axilla, but to him it is unaccountable that any man of the least pretensions to anatomical knowledge should think it in the ischiatic notch.

"When Dr. I. was first called to Lowell, his injured limb stood off from the other at an angle of about 40 degrees; a tumor was seen, and the head of the femur was felt by Dr. I. over the foramen ovale, the trochanter major of course displaced. When Dr. I. left him, the direction of the whole limb was natural, its motion (see Coffin's deposition) free, its length corresponding with the other, and the trochanter major to be felt in its natural position. Dr. I. judged Lowell's case to be, when he was called to him, a dislocation into the foramen ovale, accompanied with a fracture of the acetabulum, and after he had operated upon him, that he had reduced it. Dr. Warren saw Lowell some months afterwards, and judged that it was originally a dislocation into the ischiatic notch, and that it never had been reduced. His cogent reasons for his belief are to be found in his depositions published in the Report of the Trial.

"A correspondent of the Boston Patriot complains most bitterly of the medical aristocracy now existing, and says, 'a reform is most devoutly de-

* These productions were, it should seem, printed for a more extensive distribution, than the ordinary circulation of the paper. Many copies were sent me, mostly by anonymous friends, and a considerable part of them I found to be printed with one blank side of the newspaper, additional copies of this series being struck off for a more enlarged diffusion of their contents, and, as I have since learned, distributed by the defendant. I might add, that I have received from Maine, through the post-office, anonymous writings of an abusive character, probably the production of medical students, who had been deceived as to my opinions and dispositions, by the pamphlet of the attorney, and the newspaper publications. They afford additional proof of the disadvantages resulting from publications of the description of the report of this trial; since its allusions and insinuations seem to encourage and support those, who are ready to improve any opportunity to attack the reputation of others.

sired.' I think if it was not wished for by a liberal minded individual, that the public good demands it in a most peremptory manner. 'The truth, and the whole truth, ought to come out.' On the one hand, if Dr. I. is not fully competent to discharge the duties which belong only to the accomplished physician and surgeon, it ought to be fully developed, as he is believed, by those who are the best acquainted with him, to possess a high degree of excellence as a medical man, and let them be no longer deceived. On the other hand, if J. C. Warren should be found not worthy of the high degree of confidence that has been placed in him, in consequence of the nakedness of his mind, I think that he must take a high rank among that detestable class of men who pretend to much and know but little. If Lowell's case was a plain one, a case not to be mistaken by medical men of high standing, and if medical books are so clear and explicit on the subject, that the wayfaring man could not err therein; and if no medical authority extant intimates that the knee and limb of one side being thrown off from the other in an awkward and unnatural manner, is an indication that the head of the thigh bone thus thrown off is lodged in the ischiatic notch; and if no man of common sense ever dreamt of the impossibility, I do think the prospects of invalids, when they become inmates of the Massachusetts General Hospital, to be the most deplorable of any situation that I can imagine."

"The circumstance of their volunteering their depositions, as they certainly did, to Lowell; for every man knows that they were not compelled by law to do it, as they lived out of the State in which the action was brought, and the tenor of their depositions, as it must appear to every dispassionate reader, throws the lie in the reviewer's face. It would seem that they believed a verdict in Lowell's favour (regardless of equity towards the defendants), would prove that they did not see through glasses darkly; would prove, in fact, an impossibility, and thereby save *their* reputation."

Such are the remarks which the defendant or his friends, have felt justified in publishing. What I have quoted is by no means the most abusive part of their productions; but there being no great choice, I have taken the above at a venture, as a specimen of the rest.

That the defendant should have felt some irritation at the inconvenience he had experienced in consequence of the opinion formed by me, is perfectly natural. But a moderate degree of good feeling would have led him to restrain such publications, when he well knew from the tenor of my letter, from the four cross interrogatories, 15, 16, 17, 18, as well as from various concomitant circumstances, that I had every disposition to prevent his suffering, and that nothing would have led me, knowingly, to say what might be employed against him, but obedience to the laws of the country.

The writer of the paragraph, quoted last, boldly asserts, that we volunteered our depositions, and that we were not compelled to give them by law, because we lived in a different state. A person who undertakes to assail the character of others, with so little reserve as he has done, ought to have more knowledge of the facts of which he speaks. It is well known that citizens of the United States are compelled to give testimony in causes between citizens of different states; or become liable for all damages. How else could justice be administered in the national courts?

So far from volunteering my testimony, I positively refused it, till I found the law of the land could compel me to give it; and then, as I have more than once repeated, I gave it most reluctantly.

There is but one other part of this incongruous mass of assertion and insinuation, which I shall notice. "When Dr. ———," say these newspaper writers, "was first called to Lowell, his injured limb stood off from the other at about an angle of 40 degrees," &c.

From this and what follows it appears, that the defendant has abandoned the cause of the deponent *A.*, who says, "that from the nature of the injury described to me by Lowell, it could hardly be possible that the hip

should be dislocated. A fall on the back, with the weight of the horse upon it, would be likely to break the bones of the pelvis, and might drive the head of the bone through the bottom of the socket, but *could not dislocate* the joint." (See deposition *A*).

When it is considered, that but for the difference of opinion from us, expressed by the deponent *A.*, the defendant would probably have lost his cause, it appears very ungrateful in him to abandon, and even to attack, or allow to be attacked, the opinion, and thus indirectly the character of so important an auxiliary.

Further, in regard to the conduct of the deponent in the surgical management of the case, which is spoken of in these newspapers, I have always wished to avoid giving any opinion. I therefore never charged him with any malpractice, or with a want of as much knowledge, as should be reasonably claimed for him; nor do I now. I wish only to defend my own ground. But in the first part of the production quoted, the writer has contrived to connect the opinion of the defendant with mine in such a manner, as calls for some remark. After informing us, that the defendant considered the case of Lowell as a dislocation in the foramen ovale before he reduced it, he has made me to say, that it was *originally* a dislocation in the ischiatic notch. This is not the fact. I expressly declined giving an opinion, as to what took place at the period of the accident, and spoke only of what I saw at the time I examined the patient. There is not the least intimation of its having been originally an ischiatic dislocation.

The writer of this statement, which, as I have said, is repeated elsewhere in his productions, appears by no means to have improved the position of the defendant. It seems, from his story, that before any effort was made at reduction, the defendant was perfectly aware of the existence, and the character of this accident. The limb, he says, stood off at an angle of 40 degrees.

Attempts were made to reduce it, according to the deposition of Coffin, which is introduced by this writer as favourable to the defendant, and which, for this reason, I have reprinted. The direction of the limb, after these attempts, was changed, and "both legs were of a length, so far as I saw, and the limb was more easily moved."*

Yet the same Coffin informs us that he was present at a period from twelve to fourteen days after the injury, when, the defendant being there, "Lowell asked the reason of the hollow at the outside of the hip, and expressed his fears that the bone was out of place." Nothing was done with a view to replace it. In about two and a half months after, he was examined by me, and the bone was found dislocated; the existence of which, I suppose, to be shown by a stronger body of evidence than any adduced by this newspaper writer in support of what he has advanced. Considering, then, the condition in which the limb now is, together with the statement made by him, does not this author involve the person he defends among the following alternatives?

First, that by the attempt made for reduction, the bone was actually reduced; yet, when it afterwards got out of place *in a fit*, or in some other strange way, the defendant, though seeing the disordered state of the limb, made no attempt to restore it to its place.

Secondly. Or that when the reduction was attempted, the head of the bone was not moved from the situation in the foramen ovale, where, it is said, he found it.

Thirdly. Or that at the time of the grating and change of direction of the limb, the head of the bone was slipped from the foramen ovale to the ischiatic notch, or os ischium.

* Writers on dislocation state, that, immediately after the accident, and especially after attempts made to reduce it, the limb appears more flexible than might be expected, or than it is ever after. This fact should be a caution to young practitioners, not to be satisfied too easily with having effected the replacement.

I wish not to be understood as bringing either of these charges; but to leave the matter to the decision of the newspaper writers between themselves; and, in settling the question, they must take particularly into view what Sir Astley Cooper has said on the third case. "Although a dislocation in the foramen ovale may be occasionally reduced by attempts made in a very inappropriate direction, yet the following case will show the mischief which may occasionally arise from an error in this respect. I once saw the following instance. A boy, sixteen years old, had a dislocation of the thigh into the foramen ovale; he was placed upon his sound side, and an extension of the superior part of the thigh was made perpendicularly. The surgeon then pressed down the knee, but the thigh being at that moment advanced, the head of the bone was thrown backward, and passed into the ischiatic notch, from which situation it could not afterwards be reduced."

After a few copies of Dr. Warren's Letter to Judge Parker had been sold, Mr. Lowell, the plaintiff, called on the publisher to say, that having seen the publication, he did not consider his case fairly represented; as the Deposition first given by Josiah Coffin was not printed in the work, but one given by said Coffin twenty-six months after which varies in a very essential manner; and as an act of justice to Mr. Lowell, Dr. Warren has consented to have this printed and annexed to the work.

JOSIAH COFFIN'S FIRST DEPOSITION.

I Josiah Coffin, do give evidence, testify and declare, That being at at Lubec, in September, 1821, was at work on a house of Jabez Mowry, near the road, saw Mr. Charles Lowell when he fell from a horse and was much wounded by the horse falling upon him; that I helped carry him to his house, and was present when Dr. K. came in, heard Mr. Lowell request some person to call Dr. K. before we had got him to his house. Mr. Lowell was placed on the bed and Dr. K. said that his hip was out or dislocated. Dr. K. said that he thought that he could set it, or that there would be no difficulty in setting it, but that it would be a hard job for him to bear. *Dr. K.* then commenced the operation by placing Mr. Lowell on his back, and called upon several men that were present to assist him; *and after operating a short time, perhaps fifteen minutes, then said that the joint was in, and called upon several present to take hold of his leg*, and see that if they did not think it was in, and they generally, or all, expressed their opinion that they thought the bone was in, but I thought not, myself; and at the door told Mr. Lowell's brother that I thought that the bone was not in, any more than when he was carried into the house, and recommended the sending for Dr. I., and Mr. Lowell's brother and Mr. Stearns both expressed a wish that I should express my opinion before Mr. Lowell and Dr. K. and all present, and which I did, and Dr. K. again asked me to examine the leg, and said it must be in its place (or something like that,) and I did, and expressed my opinion from the position of the leg that the bone could not be in its place, the leg standing out in an unnatural position from the body, and could not be moved inward, without appearing to give him very great pain, but was moved outward, without appearing to increase the pain. That there was no purchase used for the extension of the limb, but several men with their hands, and the Dr. placed a ball of cloth between the legs of the patient. When the Dr. first said the leg was set, I had not heard any snapping or grating of the bones. I do not recollect whether Dr. K. bled the *patient*, either before or after the operation. I should think in about two hours or a little more, Dr. I. came over and I went with him to Mr.

Lowell's,—the patient appeared to be in considerable pain, Dr. I. proceeded to examine Mr. Lowell's hip and shook his head; he stopped a spell and walked across the room, asked Dr. K to take hold of the limb and he would be convinced.—Dr. K. then said, I did think that the bone was set, but now think that I had been mistaken. Dr. I. asked Dr. K. to go into the room, and they did go; after they came in, Dr. I. said Mr. Lowell we must hurt you again, as what has been done has done no good, (or words to that effect.) Mr. Lowell answered, that you must do what you think right, and I must bear it. Dr. I. said that he thought the socket of the hip bone was fractured. The Dr. then requested the by-standers to leave the room; after some time Mr. Lowell's brother asked me to go in, that more help was wanted, that I went in, a large cloth was placed round the patient's leg above the knee, and one or two men had hold of the cloth and I was asked to take hold of the leg below the knee: that a sheet was passed between the legs and over the shoulder of the well side, with men hold of the sheet at the shoulder, and *Dr. I. took hold of the hip and thigh*, and asked us to pull and extend the leg which we did—after which *Dr. I. took* hold of the leg and moved it in different directions, *asked Mr. Lowell if it did not feel natural to what it had done, and Mr. Lowell said that it did. Dr I. and Dr. K. then both said that the hip was well set*, or set correctly—(before I left Mr. Lowell's, *I heard Mr. Lowell say something to Dr. I. respecting coming over, that Mr. Lowell said he did not value the expense, and I think Dr. I. said that I will come over to-morrow.*)—That Dr. K. was handy and could be in every hour, or something to that effect, and asked Dr. K. if he had certain medicine. At this operation by Dr. I. and Dr. K. there was no ball of cloth or fulcrum placed between the legs. Something like a fortnight after, I was to see Mr. Lowell with Dr. I. *Mr. Lowell told Dr. I. that he had been in extreme pain, and that he feared that the bone was not in its place.* (Mr. Lowell said that he had a struggle by pain, and did not know but *that* might have been the cause of getting it out, if it had been in, and was confident that it was not in its place, that *Dr. I. replied that the bone's being fractured would cause pain, and that it would be some time before he would be free from pain; but that the bone was in its place, and that there was no doubt but that he would be as well as ever again.*) *Mr. Lowell then shewed Dr. I. that there was a hollow place at the outside of the hip joint. The Dr. replied, that when he had gathered strength so as to bear the weight of his body, that the hollow place would fill up.* Mr. Lowell stood up at *the Doctor's request, and supported himself by my shoulder and the bed-post. The Dr. then took hold of his leg and swung it and said that he did not see any thing out of the way, but that it was correct.* And further the deponent saith not.

JOSIAH COFFIN.

Opened and filed in Court, Sept. term, 1822.

J. HARRIS, *Clerk.*

Washington, ss.

On the twenty-seventh day of March, in the year of our Lord one thousand eight hundred and twenty-two, the aforesaid deponent was examined, and cautioned, and sworn, agreeable to law, to the deposition aforesaid, by him subscribed, taken at the request of CHARLES LOWELL, of Lubec, in the County aforesaid, Trader, and to be used in an action of trespass on the case now pending between him and K. of Lubec, aforesaid, Physician, and I. of Eastport, in said County, Physician, before the Circuit Court of Common Pleas, next to be holden at Machias, in said County, on the first Tuesday of September next, and the adverse party was not present, Dr. K. was notified. The said Deponent living more than thirty miles from the place of trial, is the cause of taking this deposition.

DANIEL PEASE, *Justice of the Peace.*